054

SOME CONTRIBUTIONS OF MEDICAL THEORY TO THE DISCOVERY OF THE CONSERVATION OF ENERGY PRINCIPLE DURING THE LATE 18th AND EARLY 19th CENTURIES

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ABSTRACT OF THESIS

Several scholars have investigated contributions that medical theorists made during the 19th century to the discovery of the conservation of energy. This thesis investigates such contributions, particularly in Britain and Germany, in greater detail than has been done hitherto. Beginning with ideas on power of 17th and 18th century British philosophers, the development of an interest in dynamics is traced through the writings of some two dozen British medical theorists between about 1760 and 1860. Gradually their ideas on power or force - these two words were usually synonymous - became sharper, and by the 1830s the parallel studies on force in the physical sciences were influencing them considerably. Thus, in the 1840s William Robert Grove's (1811-1896) formal enunciation of the correlation of forces seemed to give physiologists extra confidence in their ideas, especially on the correlation of physical and vital forces and the non-creatibility and indestructibility of power in the living organism. Two physiologists in particular have been discussed as illustrations of how readily the formally enunciated principles of the correlation of forces and the conservation of energy were applied to their physiology in the 1840s and 1850s.

Part II of the thesis discusses the growth of dynamical physiology in Germany, focussing on some half dozen physiologists. Since these men were influenced strongly by 18th century German philosophy, particularly by *Naturphilosophie*, a chapter has been given to sketching their philosophical heritage from Benedict Spinoza (1632-1677) to Immanuel Kant (1724-1804). Like their British contemporaries, their dynamics were often vague, but by the 1830s they too benefitted from the dynamical studies in physics. Finally, several of these German physiologists enunciated clearly their own form of the conservation principle; Julius Robert Mayer (1814-1878), Hermann Helmholtz (1821-1894) and Justus Liebig (1803-1873) are the best known of them.

The conclusion of this thesis is that there were physiologists in Britain and Germany during the late 18th and early 19th centuries whose interests in the dynamics of life crystallized in the closely related principles of the correlation of forces and the conservation of energy.

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APOLOGIA

Since a handful of lengthy studies have been done by historians of science during the last two decades on the discovery of the principle of the Conservation of Energy, it might well be asked: Why another? In order to answer that, let us enumerate those studies. One of the earliest was a monograph entitled *Concepts of force. A study in the foundations of dynamics*,[1] by Max Jammer in 1957. Jammer was interested more in what we nowadays call force rather than in energy, but his analysis was thought-provoking and broad enough to have been a useful stimulus for this Thesis. Two years later there was published T.S. Kuhn's paper on *'Energy conservation as an example of simultaneous discovery'*[2] which, in my opinion, achieved three things. Firstly, it assembled what was known already about several individual contributors to the Energy principle so as to form a quite impressive thesis, namely simultaneous discovery; secondly, as almost all scholarly studies must, it broke new ground, presenting thitherto ignored perspectives on several individuals - particularly on Justus Liebig (1803-1873); thirdly, it posed stimulating questions for further research.

Several papers came out in the 1960s that had some relevance, though often only *en passant*, to the Energy Conservation question. The next major work was Yehuda Elkana's *The discovery of the conservation of energy*[3] in

1974, and this was followed immediately by R.B. Lindsay's *Energy: historical development of the concept*.[4] Both these later studies concentrated on the contributions that mathematicians, physicists and engineers had made and, like Jammer's study, they largely ignored the possibility that medical theorists might have contributed significantly. Perhaps this is unfair to Elkana, whose study is much more scholarly and historical than Lindsay's, since he included one chapter on that aspect; but one feels that little, if any, research was done for it.

To my knowledge, no systematic study on whether medical theorists contributed significantly to the Energy conservation theory has been published. As is well known, several of the major enunciators of the theory in the 1840s were medical men, namely Julius Robert Mayer (1814-1878) and Hermann Helmholtz (1821-1894); but no-one has enquired how deeply their Energy ideas were rooted in their medical training, or whether there had been a tradition in medical theory of which Mayer and Helmholtz were the culmination. Admittedly, a dissertation was published in 1941 by one Rudolf Cierpka, entitled *Das Gesetz von der Erhaltung der Energie in seiner Auswirkung auf die Physiologie in Deutschland*,[5] but Cierpka was interested almost exclusively in the impact that the Energy principle had on physiology after its formal enunciation, that is post-1845. The earliest physiologist he mentioned was Johannes Müller (1801-1858), and that was only a mention. Cierpka's study is still worth reading but it has been superceded by more recent studies, some of which are excellent but do not concern this present one.

This thesis takes its cue from Kuhn's paper: since Energy Conservation was a classic case of simultaneous discovery, and at least two of its discoverers were medical men, should we not enquire whether other medical theorists contributed too? It would not be unreasonable to expect some positive results. The following is the fruit of such expectation.

NOTES TO APOLOGIA

1. M. Jammer, *Concepts of force*, Cambridge, Massachusetts, Harvard University Press, 1957.

2. T.S. Kuhn, 'Energy conservation as an example of simultaneous discovery', in Marshall Clagett (editor), *Critical problems in the history of science*, Madison, The university of Wisconsin Press, 1969, pp.321-356.

3. Y. Elkana, *The discovery of the conservation of energy*, London, Hutchinson Educational Ltd., 1974.

4. R.B. Lindsay, *Energy: historical development of the concept*, Stroudsburg, Pennsylvania, Dowden, Hutchinson & Ross Inc., 1975.

5. R. Cierpka, *Das Gesetz von der Erhaltung der Energie in seiner Auswirkung auf die Physiologie in Deutschland*, Berlin, Franz Linke, 1941.

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Introduction

On reading Justus Liebig's *Animal chemistry*,[1] I was struck by his preoccupation with force. Vital, chemical and physico-mechanical forces were obviously central concepts in his science; indeed so central that the passages in *Animal chemistry* where he discussed force were sometimes not far short of pure poetry. Force had fired his imagination as well as his intellect.

Whilst enquiring into Liebig's interest in force, I read some papers written by his contemporaries on animal chemistry and physiology, and also some recent historical studies on his and his physiological contemporaries' ideas on force. Especially useful among the secondary literature were papers on Liebig's vitalism by Timothy Lipman[2,3] and papers on the school of Johannes Müller by Paul Cranefield.[4,5] Also worthy of mention were the lengthier studies by June Goodfield[6] and Everett Mendelsohn.[7] One impression that emerged from reading these studies was that Liebig and others must have owed something to a dynamical tradition within physiology for their interest in force or energy. (Please see end of this introduction for a comment on the various meanings of force, energy, power and *Kraft*). If one follows up the references to medical and physiological treatises that Liebig, Helmholtz, Emil DuBois-Reymond (1818-1896), Müller and others made in their dynamical discussions, one realizes that there was a host of less well known medical figures in the late 18th and first half of the 19th century, who participated in the discussion on force and might have contributed to the idea of its conservation. Of course, most references were to German studies, but a goodly proportion was to British researches. It seemed, therefore, that a promising task would be to investigate whether the British and German medical traditions, between c.1770 and 1850, contributed significantly to the emergence of the concepts of energy, *vis-à-vis* that of force, whether they contributed to the idea of its conservation, and whether there was a significant interaction between them on the energy theme.

It was clear that merely the German and British material would be too much to handle with full satisfaction in two and a half years; this thesis is therefore far from exhaustive. As it was, the field expanded, for the medical writers seem to have been influenced significantly by the ideas on power and causality of 17th and 18th century philosophers, particularly John Locke (1632-1704), David Hume (1711-1776), Thomas Reid (1710-1796) and Dugald Stewart (1753-1828) in Britain, and Benedict Spinoza (1632-1677), Gottfried Leibniz (1646-1716), Immanuel Kant (1724-1804), Johann Fichte (1762-1814) and Friedrich Schelling (1775-1854) in Germany. Indeed, so influential were those philosophers, that it seemed necessary to allot two portions of this thesis to their ideas on power, force and causation; one portion precedes the chapters on the British physiologists; another portion precedes the chapters on German physiologists.

Most of the published material of about 40 British physiologists has been examined for this thesis. Most of them were practising physicians; often they were academics too. It shall be argued that many of their physiological ideas focussed on the forces that were believed to operate in and upon the animal economy, particularly in the fields of nutrition, animal heat, muscular motion, physical exhaustion and fever, that some of them asserted that there was a fundamental correlation among the forces of Nature and that force in general was subject neither to creation nor destruction within the living system. All this occurred before the mid 1840s, often considerably earlier. Indeed, several of those physiologists, namely William Carpenter (1813-1885), Peter Mark Roget (1779-1869), whom Kuhn discussed at some length in the first, unpublished draft of his paper,[8] Southwood Smith (1788-1861) and Orson Squire Fowler (1809-1887) arrived at the energy conservation principle independently by c.1840 or even earlier, and entertained it not merely as a likely speculation, but as a sober, well substantiated doctrine.

The chapters on German contributors focuss only on major physiologists, in contrast with the British figures of whom some were decidedly second rate, largely because of the limitation of my time. It will be argued that Christoph Wilhelm Hufeland (1762-1836), Johann Friedrich Blumenbach (1752-1840), Friedrich Tiedemann (1781-1861), Johannes Müller, Justus von Liebig, Hermann Helmholtz and Karl von Reichenbach (1788-1869), who is the only minor German figure in this thesis, all derived their ideas on energy and its conservation largely from their physiological backgrounds.

Indeed, it seems that the pressure that was exerted constantly on physiologists to explain exactly what they understood by the terms "vitality" and "vital force(s)", to justify the appellation of "science" for medicine and physiology (anatomy was obviously "scientific"), and to defend their ideas against reductionists (on which Lipman, Cranefield, Owsei Temkin[9] and Mikuláš Teich[10] have written most useful papers), forced them to think deeply upon the general concept of force or energy, and to gather their evidence from all areas of natural philosophy - indeed, from theology and natural theology too. One especially important aspect of vital force that was put under pressure was the question of purpose or teleology; as Temkin showed as long ago as 1946, physiologists like Theodore Schwann (1810-1882) who roundly denounced purpose in biology, only used downright teleological language in large sections of their own writings,[11] so that some aspects of force or causation in biology were patently impossible to treat rigorously despite the pressure to do so. Nonetheless, it seemed to some physiologists that to crack the problem of vitality, they had to crack the larger problem of force in all its forms; only by resolving the larger problem could they feel satisfied with their solution of the smaller, physiological one. Consequently, when the larger one had been solved officially by the late 1840s, some of them almost literally heaved sighs of relief and self-congratulation; instances of this were William Carpenter's long, triumphant paper 'On the mutual relations of the vital and physical forces'[12] read before the Royal Society in 1850, and Richard Fowler's lecture 'If vitality be a force having correlations with the forces, chemical affinities, motion, heat, light, electricity, magnetism, gravity, so ably shown by Professor Grove to be modifications of one and the same force',[13] which he gave in 1849 at the age of 84.

It is hardly surprising that physiologists - a breed which, in the modern sense, came into being only in the mid 18th century - should have interested themselves in dynamics. As soon as medical men focussed on process and function in the animal economy, rather than on anatomical structures, they were bound to think in terms of agents, forces and powers; force was an absolute necessity because, as Joseph Needham put it, a *deus* always had to be found for a *machina*.[14] Having posited forces, it was inevitable that their interrelations would become a subject for theorizing: Robert Whytt (1714-1766) discussed their 'sympathies' with one another, Erasmus Darwin (1731-1802) discussed their 'catenations', Johann Blumenbach discussed their derivation from the 'nisus formativus', and William Carpenter, Richard Fowler and a few others discussed their 'correlations'.[15] Other examples could be given, but the point has been made. Such physiologists also hankered after laws by which their forces acted, in the hope that they might do for physiology what the semidivine Newton had done for natural philosophy with his law of gravitation; it seemed that the organic agents could be studied profitably *vis-à-vis* the inorganic agents which drove Nature at large, the hope being that a comprehensive concept of force might thus be constructed. Several physiologists fulfilled that hope, arriving at a view of force identical to that of physical philosophers like William Robert Grove (1811-1896) and James Prescott Joule (1818-1889). Of course, to clinch the argument on force or energy, on its correlations and conservation, it had to be expressed mathematically. That was the achievement of a physiologist, Hermann Helmholtz.

This introduction would be incomplete without mentioning certain difficulties that have beset me throughout. Firstly, there has been the constant possibility that our modern understanding of an expression might not accord with its 18th century meaning; occasionally a particular expression required research on that score, and one such was 'animal oeconomy'. It is well known that by the 1830s it was being replaced by the word 'physiology', but did animal oeconomy in 1770 mean the same as physiology in 1830, so far as professional medical men were concerned? A perusal of the definitions of these two terms in medical cyclopaedias between c1770 and 1830 showed that they had very similar meanings, and that their differences are not worth mentioning; only in the political sense did the word oeconomy change significantly during that period. Another source I found useful for this particular etymological query was the popular literature of that period; for instance, Tobias Smollett (1721-1771), who was a physician as well as a novelist, used the word oeconomy in the medical content quite often in his novels,[16] and he clearly had in mind very much what an English physician in the 1830s would have by physiology.

Other expressions which required scrutiny were agent, force, power, *Kraft*, *vis viva*, *nisus*, correlation, vital force, and balance. Take this last word, which was used in the sense of an equilibrium between what an animal ingested and what it excreted; some physiologists saw this as a purely material equilibrium between the foodstuff that an animal absorbed and the dead stuff that it ejected; others saw it as a dynamical equilibrium between the chemical and physical forces that entered an animal via its food, sunlight and ambient temperature, and the forces that it released as animal heat, thought, sensation and movement. Sometimes a physiologist envisaged both material and dynamical equilibria: Liebig was a prime example of this, and it is important for the historian to understand which equilibrium he had in mind in particular passages, in other words, whether he was thinking of chemical substances or physico-chemical forces.

Another persistent problem was to understand what was meant by the words force, power, energy and *Kraft*. By the second half of the 19th century, physicists were differentiating between force as a propensity or cause within a system to undergo some activity, and the energy that a system had to possess whereby it actually acted and which was measurable solely by that activity. But even Helmholtz failed to differentiate satisfactorily between what we now call force and energy, for in 1887 the Göttingen Philosophical Faculty offered a prize for an essay clarifying the relationship between Helmholtz's *Erhaltung der Kraft* and the principle of energy conservation as it had become established by then. If Helmholtz failed, it is scarcely surprising that his predecessors in the study of *Kraft* failed too. Yet simply because of that, they should not be dismissed as non-contributors to the concept of energy, as we define it today, for even in modern physics there is an essential relation between force and energy, and in order to treat of the latter one needs to have considered the former. It seems to me to be historically inconceivable that the mid-late 19th century refinement of the concept of energy could have occurred without a prior, profound interest in the concept (however vague) of force or *Kraft*. Similarly, as Jammer[17] has asserted on the concept of force itself, primitive man's brute awareness of his own physical abilities was an awareness of a 'force' even though it could not be articulated in the form of P=m a.

This question of what was meant by force, energy, power and *Kraft* is not a merely semantic point, since the concept of the conservation of energy is truly distinctive and mathematically applicable only when energy has become differentiated from force, when energy is considered equivalent to the amount of doing that a force does, or in modern terminology, when energy has become associated with work.

Most of the physiologists discussed in this thesis did not trouble to differentiate among these words and often used them interchangeably. However, it is possible sometimes to distinguish when they meant force in action, or the quantity of its doing, from when they meant a mere propensity towards action or a *vis mortua* (to use Leibniz's phrase). One physiologist who sought such differentiation was John Brown (1735-1788) although, as we should expect, his differentiation was rough and ready and could easily be dismissed as unimportant by an historian who is not primarily interested in this aspect of his physiology.

Elkana and others have suggested that it is often better to retain the original word *Kraft* without differentiation or translation when writing in English about this period. If I were writing only about German physiologists I would follow that suggestion, but since this thesis discusses English physiologists too, and often in the same chapters as the German ones, it would be clumsy to use *Kraft* and force simultaneously; *Kraft* has therefore been translated as force or energy, and, wherever possible, the word has been chosen to accord with the apparent sense of the original context; where ambiguity has been irresolvable, the German original has been included in parentheses. There is no easy way for an historian to handle this particular etymological problem, for the interpretation by one scholar will undoubtedly be challenged eventually by another: for instance, Goodfield took great care in analysing Liebig's various uses of *Kraft* in his *Animal Chemistry*, yet Elkana has objected quite strongly to her analysis.

Finally: the development of physiologists' ideas on force was a complex historical process and much remains to be investigated. There is a vast amount of French and Italian primary sources yet to be studied; there is also the necessity of appreciating the involvement of natural theology and metaphysics in this field: for instance, philosophers, physiologists and physical scientists often wound up their discussions on force with the question of the ultimate origin of all force. Whence heat, light, electricity, magnetism, motion and vitality? Their answer was usually GOD, whose DIVINE WILL was the original power which became manifest in the physical, chemical and vital powers that inform the material world; ultimately therefore, all forces were but correlations of one another through their common origin in the DIVINE WILL, and "thy will be done on earth as it is in heaven" was not only a pious hope but also expressed the physical, dynamical dependance of the world upon God. Sober scientists saw fit to include such theological ideas in their scientific papers; we must remember that until the mid 19th century a man could be more of a natural philosopher than a scientist, and it is historically infelicitous to disregard his extra-scientific ideas. We should also not ignore the possible relevance of externalist factors to our energy theme, such as the general social interest in power and its efficient utilization in industry.

Having mentioned a few of the possible approaches to our theme of energy and its conservation, it must now be admitted that this thesis can be only a limited contribution to a large field. All that may be claimed for it is that the physiologists' interest in energy has been investigated at a depth hitherto unattempted, and that a few new insights into the energy theme might be offered to fellow historians.

NOTES TO INTRODUCTION

1. J. Liebig, *Animal chemistry, or organic chemistry in its application to physiology and pathology*, edited from the author's manuscript by W. Gregory, New York, Johnson Reprint Corporation, 1964.

2. T.O. Lipman, 'The response to Liebig's vitalism', *Bull.Hist.Med.*, 1966, 40:511-524.

3. T.O. Lipman, 'Vitalism and reductionism in Liebig's physiological thought', *Isis*, 1967, 58: 167-185.

4. P.F. Cranefield, 'The organic physics of 1847 and biophysics today', *J.Hist.Med.*, 1957, 12:407-423.

5. P.F. Cranefield, 'The philosophical and cultural interests of the biophysical movement of 1847', *J.Hist.Med.*, 1966, 21:1-7.

6. J.G. Goodfield, *The growth of scientific physiology*, London, Hutchinson, 1960.

7. E. Mendelsohn, *Heat and life: development of the theory of animal heat*, Cambridge, Mass., Harvard University Press, 1964.

8. C.B. Boyer, 'Commentary on the papers of Thomas S. Kuhn and I. Bernard Cohen', in Marshall Clagett (editor), *Critical problems in the history of science*, Madison, University of Wisconsin Press, 1969, p.387.

9. O. Temkin, 'Materialism in French and German physiology in the early 19th century', *Bull.Hist.Med.*, 1946, 20:322-7.

10. M. Teich, 'The historical foundations of modern biochemistry', in J. Needham (editor), *The chemistry of life*, Cambridge, Cambridge University Press,1970, pp.171-191.

11. *Op.cit.*, Note 9 above, p.325.

12. W.B. Carpenter, 'On the mutual relations of the vital and physical forces', *Phil.Trans.*, 1850, pp.727-758.

13. R. Fowler, 'If vitality be a force having correlations with the forces, chemical affinities, motion, heat, light, electricity, magnetism and gravity, so ably shown by Professor Grove to be modifications of one and the same force', *Rep.Brit.Assoc.Advmnt.Sci.*, 1850, pp.77-78 of the sectional transactions.

14. J. Neeham, *Science and civilization in China*, Cambridge, C.U.P., 1956, Vol.II, p.302.

15. For a useful discussion of these physiological theories, see T.S. Hall, *Ideas on life and matter*, Vol.2, Chicago, University of Chicago Press, 1969.

16. See especially Smollett's *The adventures of Peregrine Pickle*, first published 1751. On Smollett as a physician, there is a thorough biography by L.M. Knapp, *Tobias Smollett, doctor of men and manners*, Princeton, Princeton University Press, 1949, reviewed by L.G. Stevenson in *Bull.Hist.Med.*, 1950, 24:300-302.

17. *Op.cit.*, Note 1 Apologia above, pp.1-15.

CHAPTER 1. On Power and Force in 17th and 18th century British philosophy.

John Locke (1632-1704): Locke has justly been called the founder of philosophical liberalism and of empiricism in the modern theory of knowledge; in both these rôles, he exerted a strong influence in his own country and on the Continent, especially in France.[1] Here, we shall be concerned only with his theory of knowledge and almost solely with his medical manuscripts and his *An essay concerning human understanding*,[2] first published in 1690.

Several historians have studied Locke's association with leading exponents of the Mechanical Philosophy in late 17th century England.[3] Thus, it is well known that he was a close friend of Robert Boyle (1627-1691) and Thomas Sydenham (1624-1689), that he was interested in the application of Boyle's corpuscular theory to medicine, and that he was an early member of the Royal Society. However, despite some excellent studies on this issue, notably by Patrick Romanell[4] and Kenneth Dewhurst,[5] it remains a moot point whether he influenced significantly the way in which contemporary natural philosophers did their research.

The field of empirical research that we would most expect Locke to have influenced would surely be medicine, for he practised as a physician throughout his life. In 1949, a Swedish philosopher, Gunnar Aspelin, published a paper[6] in which he drew attention to the striking analogies between the writings of Locke and Sydenham. Since then, several papers have been published on Locke *quâ* physician, and one of the most interesting was Romanell's 'Locke and Sydenham',[7] in which he argued that Locke influenced Sydenham's medical theory (though, we might add, not necessarily his medical practice); one major piece of evidence for this thesis was that the first edition of Sydenham's treatise on fevers (1666) was considerably more dogmatic and less philosophically cautious than the third edition (1676), and since the two men first became well acquainted only in about 1667, Romanell suggested that it was probably Locke's criticism and advice which caused the change. That Sydenham thought highly of Locke is attested by the 'Epistle dedicatory' of the third edition, where Locke was praised as a man who "has, amongst the present generation, few equals and no superiors".[8] It has also been argued that Locke was indebted to various physicians for the growth of his empirical philosophy. Maurice Cranston pointed out[9] that Richard Lower (1631-1691) introduced him to the study of medicine, and Romanell argued that it was primarily Sydenham who kept him interested in the subject. With this latter point, one can hardly agree, since Locke filled his journals and common-place books assiduously with data relating to medicine and natural philosophy even during those years in France and Holland, when he had scarcely any contact with Sydenham. More plausible and thought-provoking are Romanell's suggestions that from Sydenham, Locke learned i) to appreciate the empirical method in actual practice, for instance, by attending 'variolous patients' together in London. (It is probable that c.1669 they were planning to write a joint treatise on smallpox which would have drawn from their clinical experiences together); and ii) that even if one cannot know the essence or ultimate cause of any disease, it is still the responsibility of the physician to attempt to cure; that one must therefore pursue the useful, even if one cannot discover the truth. Sydenham's own motto in medicine was "Whatever is useful is good";[10] and this accorded well with the general tenor of Locke's philosophy as it appeared in the *Essay*.

As a result of these historical studies, it is reasonable to propose that among the various sources for Locke's philosophy, the two following were significant: the religious source, as evidenced by Locke's *Letter concerning toleration*, and the medical; and that to understand Locke's thought-processes, we must never forget that he thought with a medical mind, in contrast to DesCartes, for instance, who philosophized with the mind of a geometer. Thus, of the four eminent natural philosophers whom he honoured in the epistle to the reader in the *Essay*, namely Boyle, Sydenham, Huygenius and Newton, the most influential was probably "the great genius of physick Dr. Sydenham", although Boyle made a strong impact on him too and continued to correspond and collaborate with him until 1691 when Boyle died, leaving his manuscript on *A general history of the air* in Locke's care for publication. As for the other two philosophers: Newton entered Locke's life almost certainly too late to affect his philosophy, and vice versa; Christiaan Huyghens (1629-1695) is more difficult to comment upon, for we know from Jean-Théophile Désaguliers (1683-1744), one of Newton's immediate circle of followers, that Locke, realizing he would never understand the mathematics in Newton's *Principia*, asked Huyghens whether the mathematics were reliable; once assured, he studied the general arguments of the *Principia* and was "the first who became a Newtonian philosopher without the help of geometry".[11]

All this shows that Locke was acquainted with leading physicians and natural philosophers, and that there must have been a rapport between him and men like Boyle and Sydenham. However, the problem of estimating the intensity of that rapport, and especially of discerning whether he exerted any lasting influence on the theories and working programmes of those men, has not yet been tackled satisfactorily. There are doubts and lacunae to be resolved: one small one, for instance, is that in his *Brief lives*, John Aubrey (1626-1697), who was a veritable magpie for biographical details of the men who moved in the same circles as did Locke, barely even mentioned him. Perhaps the best assessment we can make at present is that, as in his political philosophy,[12] so in his theory of knowledge, Locke reflected a significant portion of the spirit of his age.

We are on safer ground in discussing the impact of his empiricism on British natural philosophers in the 18th and 19th centuries, for throughout that time he was read widely and most of the physiologists, for instance, who were interested in power and causality cited his *Essay* extensively. Whether that *Essay* actually moulded their ideas, or merely bolstered what they had decided already, is impossible to say; but they undoubtedly believed that

Locke's theory of knowledge had been seminal, had invoked a new approach in natural philosophy, and well deserved citing.

One of the longest chapters in the *Essay* is that 'Of Power'.[13] It was cited frequently by later writers on power or force and, though Locke discussed only two manifestations of power, namely thinking and motion, they saw in that chapter certain key ideas of general import to their dynamics: to wit, that the creation of motion, and thus of any action, is denied by all our experiences of Nature; that we can envisage only transfer of motion, either from matter to matter or mind to matter; that if there ever appears to be creation of motion, it occurs when a mind wills a body to move, since only minds can produce corporeal motion in apparent excess over the original cause; and that we cannot acquire a rigorous notion of power itself.

This last proposition was important in subsequent discussion on dynamics. Locke proposed it because he believed that men could never know the physical nature of things, but merely the relations between ideas of things, or between ideas and things in Nature. As one Locke scholar has summarized it,[14] Locke's view of knowledge, as the perception of the agreement or disagreement of ideas, can be divided into four categories: of identity and diversity, of relation, of coexistence or necessary connexion, and of existence. Relations therefore constituted much of the data for human knowledge; indeed they were the only characteristics of Nature on which human understanding could acquire any certitude. As Locke wrote,

> "This further may be considered concerning relation, that though it be not contained in the real existence of things, but something extraneous and super-induced, yet the ideas which relative words stand for are often clearer and more distinct than of those substances to which they do belong. ... The ideas, then, of relations are capable of at least being more perfect and more distinct in our minds, than those of substances".[15]

This spirit of agnosticism applied equally to the question of causality, for although Locke believed in the reality of causal relations, he doubted that the nature of any particular cause could be known. In one place,[16] he wrote of causality being the most comprehensive relation that could exist between ideas in the human mind; thus the agent of causality, namely power, was one of the most important of man's primary or original ideas. Power was therefore a pivotal point in Locke's effort to discover the limitations of human understanding; it was also to become so for natural philosophers in the future. However, Locke himself did not attempt to investigate the rôle of power in natural philosophy, since his concern, *quâ* philosopher, was to study the psychological mechanism whereby man acquires knowledge, and not to investigate the physical causes of man's ideas or to study any particular branch of natural philosophy. He said this explicitly in his chapter on power:

> "I shall not, contrary to the design of this *Essay*, set myself to inquire philosophically into the peculiar constitution of bodies, and the configuration of parts, whereby they have the power to produce in us the ideas of their sensible qualities".[17]

Throughout the 18th and first half of the 19th centuries, those who discussed the relations between physical, chemical and vital agents cited Locke as one of their authorities for disclaiming knowledge of such agents in themselves, for studying relations rather than things, and for their ideas on causality. When some of them arrived at the theory of the correlation of all forces one to another, they saw themselves fulfilling Locke's injuction to study relations rather than things and were convinced that their theory could be valid despite their ignorance on the essential natures of forces.

The principle of causality was to be debated by most of the writers on power or force for the following reason: if two agents or phenomena always appear concomitantly, as do heat and light in sunlight, or life and heat in animals, one could ask whether they are two distinct agents, or whether one is the cause of the other. If the latter is true, how can it be proved? A third possibility is that those two agents might themselves be the effects of a more fundamental agent, in which case all three might be said to be correlated. Such questions *vis-à-vis* heat and light taxed James Hutton (1726-1795), the physician-turned-geologist whose *Dissertation upon the philosophy of light, heat and fire*[18] was regarded quite highly in the early 19th century in Britain; they were discussed too by the young Humphrey

Davy (1778-1829) at Bristol, and by John William Draper (1811-1882) at New York, whose writings on plant and animal physiology were concerned greatly with energy and its conservation. Perhaps the most explicit use of the philosophic discussion on causality by a contributor to the energy conservation theory was Mayer; "*causa aequat effectum*"[19] was a foundation stone of his theory.

In the next few pages we shall examine briefly one philosopher whose ideas on power and causation were essentially the same as Locke's, namely Isaac Newton, one whose ideas challenged Locke's, namely Hume, and two philosophers who set themselves the task of refuting Hume.

Isaac Newton (1642-1727): British physiologists, anxious to establish their subject on a rigorous, quantitative basis, looked constantly to the example of Newton whose mathematical laws and physical experments had given such assurance to astronomy and optics. Indeed, by the mid 18th century, Newton had become the figure of inspiration for all branches of natural philosophy in Britain, and his words had acquired the potency of oracles. This was especially true of the 'Quaeries' that were appended to his *Opticks*,[20] in which he had felt safe enough to air his many, and changing, ideas on the ultimate structure of matter and the means whereby it moved, means which seemed sometimes to be an imponderable ether and at other times to be pure forces. Newton's 'Quaeries' and the evolution of his ideas on ether and force have been analysed by several scholars, and I am particularly indebted to A.R.

and M.B. Hall's work.[21]

To their analyses I cannot add anything at all worthwhile; but it is worth emphasizing how seriously some physiologists took certain passages in the 'Quaeries' which had an especial appeal to medicine, and that from the late 18th century onwards they quoted those passages as authorizing their interest in force. The following passages in the 'Quaeries' and *Principia* were quoted most often, and their physiological relevance is obvious.

> "And now we might add something concerning a certain most subtle spirit which pervades and lies hid in all gross bodies; by the force and action of which spirit the particles of bodies attract one another at near distances, and cohere if contiguous; and electric bodies operate to greater distances, as well repelling as attracting the neighbouring corpuscles; ... and all sensation is excited, and the members of animal bodies move at the command of the will, namely by the vibrations of this spirit, mutually propagated along the solid filaments of the nerves, from the outward organs of sense to the brain, and from the brain into the muscles".[22]

> "Have not the small particles of bodies powers, virtues or forces, by which they act at a distance, not only upon the rays of light for reflecting, refracting and inflecting them, but also upon one another for producing a great part of the phaenomena /sic/ of Nature? For its well known that bodies act upon one another by the attractions of gravity, magnetism, and electricity; and these instances shew the tenor

> and course of Nature, and make it not improbable that there may be more attractive powers than these".[23]

> "There are therefore agents in Nature able to make the particles of bodies stick together by very strong attractions. And it is the business of Experimental Philosophy to find them out".[24]

In his long and immensely influential quaery 31, when discussing the phenomenon we nowadays call capillary attraction, Newton suggested that by the same principle, "a sponge sucks in water, and the glands in the bodies of animals, according to their several natures and dispositions, suck in various juices from the blood".[25] This concerned the processes of secretion and absorption which were such important themes in 17th and 18th century medicine, and for which all physiologists busily sought explanatory mechanisms. Generally, by the mid 18th century, physiologists were no longer employing Newton's rather rough and ready physiological speculations in their own theories without considerable modification; they took him for their general guide, rather than as their arbiter on finer points. But, as Arnold Thackray has shown,[26] there were medical theorists in the late 17th and early 18th centuries who modelled their systems explicitly and in detail upon his published work - men like Archibald Pitcairne (1652-1713), George Cheyne (1671-1743), Richard Mead (1673-1754), James Keill (1673-1719) and John Freind (1675-1728). Perhaps the best known physician to take Newton as his guide and mentor was

Hermann Boerhaave (1668-1738). Throughout the 18th century, medical writers cited Newton's writings enthusiastically and obviously wished to emulate him by discovering mathematical relations and fundamental forces for the animal oeconomy. A few physiologists were fair mathematicians, one of the most able being Peter Mark Roget who was a central figure in Britain in enunciating the conservation of energy (see chapter 6) and who, though a practising physician, was elected to the Royal Society of London on the strength of a very elegant mathematical paper.

<u>David Hume (1711-1776)</u>: Like Locke, Hume made the analyses of power, causality and relation important aspects of his philosophy. Like Locke, he denied the possibility of understanding the essential nature of any power or any causal relation; unlike Locke, he was sceptical of even the objective reality of power and causality and, to the horror of most of his contempories, seemed to reduce all things to being mere perceptions, and man himself to being a mere bundle of perceptions.

Hume's arguments appeared in several major philosophic treatises, in his *History of England*[27] and in various letters, some of which were published during his life. Here, we shall deal largely with his two principal philosophical works, the *Treatise of human nature*[28] (1739 and 1740), and *An inquiry concerning human understanding*[29] (1777). In the *Inquiry*, three out of the twelve sections were devoted to power and causality; the first attack was on causality which, he argued, was merely a pattern of thought in man's mind; so far as the real world was concerned, therefore, every effect was a distinct event from what man arbitrarily denominated its cause, and the causes of natural phenomena,

> "these ultimate springs and principles, are totally shut up from human curiosity and inquiry. Elasticity, gravity, cohesion of parts, communication of motion by impulse - these are probably the ultimate causes and principles which we shall ever discover in nature ...."[30]

In his assertion of the inscrutability of gravity and other powers, Hume was following Newton explicitly. One Hume scholar, Norman Kemp Smith, has shown that of the half dozen or so philosophers who influenced Hume significantly, one was Newton.[31] To take but two examples: Firstly, Hume modelled his method on Newton's in his proposal to develop a statics and dynamics of the mind, in which the association of ideas was envisaged as

> "A kind of ATTRACTION, which in the mental world will be found to have as extraordinary effects as in the natural, and to shew itself in as many and as various forms".[32]

By means of his statics and dynamics, Hume claimed to be able to give a naturalistic, mechanistic account of the human constitution and ethics, a claim that he never fulfilled. (Needless to say, Newton would not have condoned his attempt, for in the closing words of the *Opticks* he had asserted that a religiously inspired ethics was the sole legitimate supplement to his natural philosophy).

Secondly, Hume's ideas on ultimate causes agreed with Francis Hutcheson's (1694-1746) manner of stating ethical problems; Hutcheson, who had taught moral philosophy at Glasgow and published several major treatises in the 1720s, asserted that in the moral as well as the physical sphere there are certain ultimate experiences, and that although they rest on conditions of human nature which are not discernible by us, they may be considered by us as effectively ultimate.[33] As this illustrates, Hutcheson had been influenced greatly by Newton's epistemology; particularly influential was Newton's Quaery 31, where he had contrasted natural phenomena with their hidden and occult causes:

> "These principles (such as inertia, gravity, cohesion of bodie) I consider, not as occult qualities, supposed to result from the specifick forms of things, but as general laws of Nature, by which the things themselves are formed; their truth appearing to us by phaenomena, though their causes be not yet discovered. For these are manifest qualities, and their causes only are occult".[34]

As Kemp Smith has shown, Hutcheson exerted a powerful influence on Hume, and Hume's formulation of his own method in the *Treatise* obviously had both him and Newton in mind, particularly on power and causality. Though Hume did not acknowledge his intellectual debts very readily, he lavished praise on Newton in his *History of England*: there we see what the crux of Newton's achievement was, in Hume's opinion:

> "Cautious in admitting no principles, but such as were founded on experiment; but resolute to adopt every such principle, however new or unusual".[35]

Returning to Hume's own conclusions, there is a forceful and wonderfully succinct statement of the problem of power in the *Treatise*; it was simply that

> "There are no ideas which occur in metaphysics more obscure and uncertain than those of 'power', 'force', 'energy' or 'necessary connection', of which it is every moment necessary for us to treat in all our disquisitions".[36]

Hume attempted to fix the exact meanings of these ideas, failed, and in so doing taught an important lesson about power and force: namely, that our ideas of them derive essentially from our own sensations and exertions, (what Hume called collectively, our 'passions'); that the first idea of power that man possessed was the power of himself to do things; and that so persuasive was this idea, that man extrapolated it into the world around him. Therefore, power could not be said to exist outside of ourselves; the objective reality of power was therefore denied. On the other hand, since the sole phenomena of which men could be sure were their perceptions, and since power was a perception, power had as much reality as anything else.

As we might imagine, there were theological implications of Hume's critique on power, which he discussed unflinchingly; for instance, that one of the objects to which men attribute power unphilosophically is God, and that men's ideas of Divine Power, providence and God himself are mere extrapolations from human experiences. Against such passages many natural philosophers, as well as theologians, objected, and in formulating their objections they were obliged to think deeply on power, causality and allied issues. One English scholar in the 19th century, Leslie Stephen, discussed Hume's influence beautifully: having mentioned how his first book fell "still-born from the press", and that the great dictators of the 18th century literary world, men like William Warburton (1698-1779) and Samuel Johnson (1709-1784) castigated him mercilessly, he added that

> "If Hume impressed men of mark so slightly, we are tempted to doubt whether he can have affected the main current of thought. Yet, as we study the remarkable change in the whole tone and substance of our literature which synchronized with the appearance of Hume's writings, it is difficult to resist the impression that there is some causal relation ....
>
> The explanation of the apparent contradiction must doubtless be sought partly in the fact that Hume influenced a powerful though a small class. He appealed to a few thinkers, who might be considered as the brain of the social organism; and the effects were gradually propagated to the extremities of the system".[37]

In his *Treatise*, Hume gave only one section to a thorough analysis of power and causality; that was section 14, 'Of the idea of necessary connection'. There, he called the question of power and efficacy of causes one of the most sublime questions in philosophy and all the sciences; it was an intricate question, since the terms of efficacy, agency, power, force, energy, necessity, connection and productive quality seemed to him to be nearly synonymous and therefore useless for defining one another. The only solution he could admit would be an experimental one, the task being to discover some natural effect where the operation of a cause could be conceived clearly without any danger of obscurity or mistake. However, since no-one had succeeded in this Newtonian task, Hume's conclusion was devastating:

> "All ideas are derived from and represent impressions. We never have any impression that contains any power or efficacy. We never, therefore, have any idea of power".[38]

As he pointed out, this conclusion differed from Locke's. Whereas Locke had accepted that men could have a valid idea of power, though no idea of the essential nature of any particular power, Hume denied the possibility even of the former; he discussed Locke's argument, only to dismiss it as "more popular than philosophical", for reason alone could never give rise to an original idea, and reason, as distinct from experience, could never demonstrate that a cause was absolutely requisite for each event of coming-to-be. Hume declared that

> "As the necessity which makes two times two equal to four, or three angles of a triangle equal to two right ones, lies only in the act of the understanding, by which we consider and compare these ideas; in like manner, the necessity of power, which unites causes and effects, lies in the determination of the mind to pass from the one to the other. The

> efficacy or energy of causes is neither placed in the causes themselves, nor in the Deity, nor in the concurrence of these two principles; but belongs entirely to the soul, which considers the union of two or more objects in all past instances. It is here that the real power of causes is placed, along with their connection and necessity".[39]

In this and other passages, Hume seems to have differentiated between a force or cause on the one hand, and an energy or power on the other. Thus, despite his above-mentioned warning that such terms are nearly synonymous in their common usage, he at least seems to have envisaged within the terms of power and energy the concept of doing or activity; for him, power was not synonymous with force but was that "which unites causes /i.e. forces/ and effects".

Hume's *Treatise* may have fallen "still-born from the press", but those who came after him could not afford to ignore it; this applied equally to many natural philosophers as to philosophers proper. Those who abhorred his ideas could do three things: they could dismiss him as an atheist; they could scrutinize his arguments to discover his weak points, as did the two Scottish philosophers, Thomas Reid (1710-1796) and Dugald Stewart (1753-1828); or they could hope that their own researches would show that forces, for instance, do exist, and that power and causality are real. The physiologists obtained a bumper crop of forces or powers; yet some of them heeded Hume's critique and would not call their forces real, preferring to leave the questions of their nature and number open. What Hume's influence on natural philosophy was has not been gauged. Yet he did influence workers in the late 18th and early 19th centuries, if only by being read widely. Take George Calvert Holland (1801-1865) who wrote several quite respected physiological treatises between c.1820 and 1835; he cited Hume for his own physiological method:

> "Here, then, is the only expedient from which we can hope for success in our philosophical researches; to leave the tedious lingering method, which we have hitherto followed; and instead of taking now or then a castle or a village on the frontier, to march up directly to the capital or centre of the sciences, to human nature itself; which, being once masters of, we may everywhere else hope for an easy victory. From this station we may extend our conquests over all those sciences which more intimately concern human life, and may afterwards proceed at leisure, to discover more fully those which are the objects of pure curiosity".[40]

Holland certainly did his physiology as Hume recommends here for the study of human nature - which must be why Holland soon fell into oblivion. However, Holland's ideas did stimulate a few major physiologists like William Carpenter and John William Draper, who were deeply involved in the question of energy and its conservation.

The question of Hume's influence on 19th century natural philosophy is complicated by several factors, of which two might be mentioned here. Firstly, there was a considerable number of writers who, though their piety or sense of sobriety, could not admit any debt to him, for his philosophy was considered generally atheistic and even dangerous; besides, there was reason to doubt that he had been in a suitable frame of mind when composing his works, for in Book 1, Part 4 of his *Treatise* he had written of his "philosophical melancholy and delirium", and of his finding no remedy against the "chimeras" of the study except in yielding himself to the carefree pursuits of everyday life. How often throughout the late 18th and early 19th centuries, his critics gleefully quoted his confession:

> "I dine, I play a game of back-gammon, I converse, and am merry with my friends; and when ... I would return to those speculations, they appear so cold and strained and ridiculous, that I cannot find in my heart to enter into them any farther".[41]

The intention of those critics, such as James Beattie (1735-1803) (who, paradoxically, drew certain important passages of the *Treatise* to Kant's attention), was to depict Hume as having taught that man must believe one thing by instinct or custom, yet the contrary by reason; that the human understanding subverts itself entirely, and that by argument nothing can be proved.[42] Need it be said that they misrepresented Hume? For he had argued that man's understanding is fed by both reason and custom, but that custom is king.

Secondly, Hume's impact on the 19th century was complicated by his own dissatisfaction with the *Treatise* and the consequent omission of several of its themes when he came to compose the *Inquiry*. The latter was undoubtedly the maturer presentation of his ideas, yet one theme he omitted was the causal axiom. We know now that he did not alter his views on causality significantly between the two treatises, and that his omission was more accidental then deliberate; but such omissions could confound his readers' understanding of his work, as indeed happened with Kant; Kant, though long acquainted with the *Inquiry*, was not able to trace for himself Hume's far-reaching consequences on certain themes, of which causality was one; and only by reading Beattie's *Essay*, where the relevant passage from the *Treatise* was quoted, did he reappraise Hume.

Thomas Reid (1710-1796): Hume's *Treatise* is a difficult and puzzling work. The youthful ardour of mind and variability of mood in which it was written, its loose terminology and other flaws, made it open to criticism as soon as people took note of it. One of its earliest, major critics was Thomas Reid.

Reid had studied philosophy, mathematics and theology, was ordained, and in 1752 received the chair of philosophy at Aberdeen. There, he set about tackling the challenges that Hume had set, and in 1764 his first important refutation of Hume was published as an *Inquiry into the human mind*.[43] Unlike some of Hume's critics, Reid acknowledged an enormous debt to him, writing in the Dedication to his *Inquiry* that

> "... I never thought of calling in question the principles commonly received with regard to the human understanding, until the Treatise

of human nature was published in 1739".

Reid's analysis of hypotheses, the human mind, power, force and causality came to be cited frequently by British philosophers, natural philosophers and medical theorists. His study of sense perception was to be regarded especially important for, as one dictionary of philosophy and psychology in 1901 put it,[44] he was the first English writer to attempt a precise definition of perception, and his views, so far as they were psychological and not epistemological, agreed substantially with those of modern psychologists.

Reid's main criticism of Hume concerned the latter's doctrine of ideas. As Kemp Smith and others have shown, the opening section of Hume's *Treatise* was a modified exposition of Locke's theory of ideas, which Hume went on to incorporate into his own system. Reid, and in more popular form, his colleague James Beattie, depicted Hume as having done no more than deliver his successors from a bondage concerning ideas, to which he himself remained bound. Hume, who was eulogized elsewhere by Reid as having been so analytical and thorough, had supposedly constructed his *Treatise* on a foundation which he had not been bothered to examine. Reid's view of that foundation was that three laws of association of ideas, joined to a few original feelings, were used to explain the whole mechanism of sense, imagination, memory, belief and passions of the mind. At first sight, he admitted, it was plausible:

> "It shows tolerably by candle-light; but, brought into clear day, and taken to pieces, it will appear to be a man made with mortar and a trowel .... I see myself, and the

> whole frame of Nature, shrink into fleeting ideas, which, like Epicurus's atoms, dance about in emptiness".[45]

Reid's interpretation of Hume's *Treatise* gained wider currency through Beattie's *Essay on the nature and immutability of truth in opposition to sophistry and scepticism* (1770), which went through twelve editions during its first decade and was twice translated into German before the end of the century. More than any other work, it has determined the common conception of Hume's philosophy.[46]

Turning now to Reid's discussions of power, causality and related themes, power features prominently in several of his essays; it was divided into several categories - the power of man to think and do, God's power to will and do, and the powers of Nature. His essays most concerned with power were 'Of active power in general',[47] 'Of the liberty of moral agents'[48] and 'Essay on quantity'.[49] He was one of the clearest exponents of the theological aspect of the powers of Nature, on which physiologists *inter alia* were to cite him not infrequently.

Inevitably, his ideas seem often to be identical to Locke's and Hume's. For instance, he was reluctant to define power, since it was a thing so much of its own kind, and so simple in its nature, as not to admit of logical definition.[50] Like Hume, he recognized the defects in Locke's investigations, but he stopped well short of Hume's extremes: for instance, in 'Of active power in general', having denied that power can ever be an object of the senses or consciousness, he still admitted its existence behind the scene. His argument was briefly that men can be conscious of a conception or idea of power, even though they might never be conscious of possessing power. Hence

> "I cannot help repeating my apology for insisting so long in the refutation of so great an absurdity. It is a capital doctrine in a late celebrated system of human nature, that we have no idea of power, not even in the Diety; ... To support this important doctrine, and the outworks that are raised in its defense, a great part of the first volume of the Treatise of human nature is employed".[51]

Reid's task concerning power, against Hume, was to demonstrate that men can properly possess ideas of power and cause, and that such ideas are so obviously inherent in the nature of the human mind that they precede whatever we might obtain by sense-perceptions. Such agents are real, he argued, though inscrutable; and to bolster his arguments, it was given a strong theological connotation:

> "But as to the real causes of the phenomena of Nature, bow little do we know! All our knowledge of things external must be grounded upon the information of our senses; but causation and active power are not objects of sense; ... It is to this day problematical, whether all the phenomena of the material system be produced by the immediate operation of the First Cause, according to the laws which his wisdom determined, or whether subordinate causes are employed by him in the operations of Nature; and if they be, what their nature, their number, and their different offices are?

> And whether in all cases they act by commission, or in some, according to their discretion?"[52]

Reid's importance in the debate on power, so far as British physiologists at least were concerned, was that he was the first powerfully to refute Hume and to uphold the validity of powers and forces. His work was regarded well: for instance, his earliest published paper (1748), was an attempt at reconciling Leibnizean and Newtonian dynamics, on which Sir William Hamilton, who edited a complete collection of his work in 1803, commented

> "... it is curious that Kant should, in the preceding year, have ushered into the world his first regular work, and on a similar subject; that work, too, containing a refutation of the Leibnizean estimate of velocity".[53]

Of other similarities between Reid and Kant, Hamilton asserted that each, in a different sphere, "was at the head of a great scientific determination", and that both were distinguished rather for their philosophical ingenuity and independence, than for the extent of their philosophical learning.

From the point of view of this thesis, namely our interest in the emergence of a fairly precise concept of energy and its laws, Reid is also noteworthy for his attempts, though unsophisticated by modern standards, to differentiate among force, power and activity. For example,

> "Our conception of power is relative to its exertions or effects. Power is one thing, its exertion is another. It is true there

> can be no exertion without power; but there may be power which is not exerted".[54]

These attempts to differentiate were taken up by Reid's pupil, Dugald Stewart.

Dugald Stewart (1752-1828): Stewart was cited frequently by British physiologists in the first half of the 19th century. He was a prominent academic figure in Edinburgh for much of his life, succeeding Adam Ferguson to the chair of moral philosophy in Edinburgh in 1785 and holding it until his death. Having studied under Reid in Glasgow, where Reid held the moral philosophy chair, his philosophy reflected Reid's and it comes as no surprise, therefore, that Sir William Hamilton also edited Stewart's works - a magnificent ten volume collection, first published 1855.[55]

Despite his allegiance to Reid, there were significant differences between them. Firstly, Stewart, being of the next generation to Reid, could discuss newer developments in philosophy and natural philosophy. Secondly, he examined certain developments within the physical and medical sciences which were to influence greatly scientific development. The most significant of these were the hypotheses of Roger Joseph Bosovich (1711-1787), James Hutton, David Hartley (1705-1757), Joseph Priestley (1733-1804) and Erasmus Darwin (1731-1802).

To review the similarities between Stewart's and Reid's ideas on power and force, Stewart's argument occurred succinctly in his treatise on *The philosophy of the active and moral powers of man*,[56] Book 3. There, he defended the reality of the active powers of man and of Nature against Hume's denial of them and of causality. His argument was that, simply because we cannot trace our idea of power to any of our senses does not imply that power is non-existent; the question to be asked was whether we annex any notion to power, different from that of constant succession; he decided that these terms are not synonymous and also followed Reid in distinguishing between power and force:

> "It must, indeed, be acknowledged, that after having had experience of our own *power*, we come to associate the idea of *force*, or of an animal *nisus*, with that of *cause*; and hence, some have been led to suppose that our only idea of *cause* is derived from our bodily exertions.... The idea of *cause*, however, and of *power*, are more general than that of *force*, and might have been acquired although we had never been conscious of any bodily exertions whatever. There is surely no impropriety in saying that the mind has *power* over the train of its ideas, and over its various faculties, as well as over the members of the body".[57]

Stewart's defence of causality was curious and important: he took Hume's argument against it, asserted that Hume should have distinguished between metaphysical or efficient causes on the one hand, and physical causes on the other, and declared that then Hume's argument would actually support causality by keeping the Deity always in view, not only as the first cause, but also as the constantly operating efficient cause in the material world.[58]

Stewart was cited often by 19th century physiologists for his defence of power and causality and his discussion of the theological aspect of power, which he developed to its ultimate conclusion by asserting that the most important feature of power is its derivation from God, and that all exertions of power in man and Nature are basically acts of God. Stewart felt obliged to mention various opponents to his idea - Aristotle, Lucretius, DesCartes, Leibniz, Boyle and Cudworth - but he could cite two powerful allies - Samuel Clarke and Alexander Pope (1688-1744). Clarke had said that all natural phenomena are done either immediately by God, or mediately via intelligent agents; that matter, being devoid of any active principles itself and possessing only one principle of motion, namely inertia, it was absurd to attribute the laws of gravitation etc., to any source other than God's continuously acting on matter.[59] Pope put this more elegantly in his oft-quoted lines that

> "All are but parts of one stupendous whole,
> Whose body Nature is, and God the soul;
> That changed through all, and yet in all the same,
> Great in the earth as in th' etherial frame,
> Warms in the sun, refreshes in the breeze,
> Glows in the stars, and blossoms in the trees;
> Lives through all life, extends through all extent,
> Spreads undivided, operates unspent".[60]

Turning to Stewart's discussions of recent theories in natural philosophy, the most important was probably that of Boscovich. Boscovich, a Jesuit, diplomatist, mathematician, astronomer and poet, had published a new theory of natural philosophy in 1758, in which he accounted for all natural phenomena by two types of force - attraction and repulsion - that operated between atoms, atoms which were mere mathematical points rather like Leibniz's monads. Furthermore, he constructed a mathematical model whereby the attractions and repulsions were reduced to being only different forms of a single force, which alone acted throughout Nature. Boscovich saw his theory as a logical development of Newton's ideas, for Newton had written

> "And as in algebra, where affirmative quantities vanish and cease, there negative ones begin; so in mechanicks, where attraction ceases, there a repulsive virtue ought to succeed".[61]

Boscovich also took heed of Newton's first "*regula philosophandi*" in the second and subsequent editions of *Principia*, where he had asserted that philosophers ought to admit no more causes of natural things than are both true and sufficient to explain their appearances, for Nature is simple.

Since Boscovich's theory was elegant, simple and of thoroughly Newtonian pedigree, it became influential among the cream of the natural philosophers in the late 18th and first half of the 19th centuries; it never became popular among second rate theorists. To the lists of scholars whom he influenced, given by Whyte[62] and Thackray,[63] I would add James Hutton, John Playfair (1748-1819) and several physiologists: for instance, one Samuel Farr (1741-1795) published a curious work in 1771 called *A philosophical enquiry into the nature, origin and extent of animal motion*[64] which was quoted not infrequently by physiologists in the early 19^{th} century, and which had strong Boscovichean undertones. Boscovich's belief in force (indeed, that was all he believed in), accorded well with Stewart's own philosophy, which was probably why Stewart discussed him with such evident regard in his dissertation 'On the idealism of Berkeley'.[65] It would be interesting to know if Stewart read Boscovich's *Theoria* before formulating his own ideas on power and force. At any rate, Stewart held him in high regard, called him "that profound and original philosopher", and in addition to his *Theoria* had read his *Supplements* to the didactic poem *De Systemate Mundi* of Benedictus Stay, from which he gleaned Boscovich's more metaphysical ideas.[66]

Two especially interesting features of Stewart's discussion of Boscovich are i) his implication that even by c.1810, his theory was scarcely known in Britain, except by an inner circle of natural philosophers.[67] (This raises the question of whether Stewart's discussion of it actually helped it to become better known). And ii) his discussion of James Hutton, who had not only done seminal work in geology but had written two profound treatises on light and heat, and one on philosophy, all of which evinced Boscovichean influence. Stewart admitted that

> "In the foregoing remarks on Boscovich's theory, considered in contrast with that of Dr. Berkeley, I have had an eye chiefly to some speculations of the late Dr. Hutton, a philosopher eminently distinguished by originality of thought ...."[68]

He examined Hutton's ideas on hardness and incompressibility, which Hutton had thought agreed with Berkeley's metaphysical argument against the existence of things external to mind; Stewart asserted that between the physical arguments that Hutton and Boscovich had used, and the metaphysical ones of Berkeley, there was actually no similarity, and therefore that Hutton's and Boscovich's championings of force were acceptable; on the other hand, Berkeley's philosophy was not acceptable to Stewart, for it "tends to unhinge the whole frame of human understanding".[69]

The question of Stewart's influence on his successors is difficult to answer. According to the *Dictionary of national biography*,[70] he was one of the most regarded and influential academics of his day in Britain. His influence was partly due to his affiliation to Reid; together they provided formidable opposition to Hume's sceptical philosophy, and as 'empiricists' in philosophy they appealed quite naturally to a fair proportion of natural philosophers, which was largely why they were cited by many of the 19th century physiologists in this thesis. Stewart's influence was also due to his personal charm and eloquence - Henry Cockburn (1779-1854), said that "there was eloquence in his very spitting", and James Mill (1773-1836), though opposed to his philosophy, declared that neither Pitt nor Fox were nearly so eloquent. In political economy he exerted considerable influence, for Sydney Smith (1771-1845), Frances Horner (1778-1817), and Lords Henry Brougham (1778-1868), Henry Palmerston (1784-1865), Henry Petty (1780-1863) and John Russell (1792-1878) were pupils of his, and all went on to become eminent public figures. However, a man's personal charm dies with him, and by the mid 19^{th} century Dugald Stewart's philosophy was becoming rapidly forgotten.

Summary

The five philosophers discussed in this chapter were quoted often by physiologists for their treatment of power, force and causality, but often for different reasons. Locke was praised as the first to analyse the human understanding critically and usefully. Newton was quoted often, not only because he had demonstrated, more clearly than anyone else, that a particular force existed, but also because of the fertility of his ideas, especially the 'Quaeries'. Hume was quoted with reservations, for despite his obvious skill, it was thought generally that he had gone too far in denying both the reality of power and the usefulness even of the concepts of power and causality. Hume made many natural philosophers uncomfortable, for they believed that any system of natural philosophy had to employ power and force, if only as heuristic models. Reid was regarded as the first vigorous champion of those models against Hume's scepticism, and Dugald Stewart was seen as consolidating Reid's arguments. In addition to his pupils mentioned above, Stewart was well acquainted with several young physiologists who were to become quite eminent; William Pulteney Alison (1790-1859) was professor of physiology at Edinburgh during his career there, and at least three of his pupils who went on to contribute significantly to 19^{th} century medical science attended and thought highly of Stewart's lectures. Those three were William Benjamin Carpenter, Peter Mark Roget and John Reid (1809-1849). Reid became a fine experimental physiologist, and might have done more but for his early death.[71] Carpenter and Roget shall be discussed later, for they were to contribute considerably to the energy conservation theory.

NOTES TO CHAPTER 1.

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3. K. Dewhurst, *John Locke, physician and philosopher*, London, The Wellcome Historical Medical Library, 1963.
   I have also found the following studies useful:
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   M. Cranston, *John Locke*, London, Longmans, Green & Co., 1957.

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   Also 'Locke's essay on respiration', *Bull.Hist.Med.*, 1960, 34:257-273.
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6. G. Aspelin, 'Locke and Sydenham', *Theoria*, 1949, 15.

7. *Op.cit.*, Note 4 above.

8. R.G. Latham, *The works of Thomas Sydenham*, London, Sydenham Society, 1848, Vol.1, p.6.

9. Cranston, *op.cit.*, Note 3 above, p.40.

10. Latham, *op.cit.*Note 8 above, Vol.1, p.24.

11. J-T. Désaguliers, *Course of experimental philosophy*, Vol. 1, London, A. Millar, 1763, p.viii.

12. *Op.cit.*, Note 1 above, p.584.

13. *Op.cit.*,Note 2 above, Book 2, chap.21.

14. A.D. Woozley, in his introduction to *ibid.*, p.45.

15. *Ibid.*, Book 2, chap. 25, sec.8.

16. *Ibid.*, Book 2, chap. 25, sec. 11.

17. *Ibid.*, Book 2, chap. 21, sec. 73.

18. J. Hutton, *A dissertation upon the philosophy of light, heat and fire*, Edinburgh, 1794.

19. See especially J.R. Mayer, 'Bemerkungen über die Kräfte der unbelebten Natur', *Liebigs Annalen*, 1842, 42:239. Also his 1845 paper on metabolism.

20. I. Newton, *Opticks or a treatise of the reflections, refractions, inflections and colours of light*, New York, Dover Publications Inc., 1952.

21. M. Boas, 'Structure of matter and chemical theory in the seventeenth and eighteenth centuries' in M. Clagett (editor), *Critical problems in the history of science*, Madison, University of Wisconsin Press, 1959.
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24. *Ibid.*, p.394.

25. *Ibid.*, p.392.

26. A. Thackray, *Atoms and powers*, Cambridge,Mass., Harvard University Press, 1970.

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28. D. Hume, *A treatise of human nature*, edited by L.A. Selby-Bigge, Oxford, Clarendon Press, 1896.

29. D.Hume, *An inquiry concerning human understanding*, edited by C.W. Hendel, New York, The Liberal Arts Press, 1957.

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32. *Op.cit.*, Note 28 above, Book 1, part 1, sec. 4.

33. *Op.cit.*, Note 24 above, pp.23-51 for an account of Hutcheson's influence on Hume.
See also F. Hutcheson, *Illustrations on the moral sense*, edited by B. Peach, Cambridge, Mass., Harvard University Press, 1971.

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35. *Op.cit.*, Note 20 above, chap. 71.

36. *Op.cit.*, Note 28 above, Book 1, part 1, sec. 7.

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39. *Op.cit.*, Note 28 above, Book 1, part 3, sec. 14.

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46. *Op.cit.*, Note 31 above, p.6.

47. *Op.cit.*, Note 43 above, pp.512-527.

48. *Ibid.*, pp.599-632.

49. *Ibid.*, pp.715-719.

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59. *Ibid.*, vol. 7, p.28.
Clarke's own words can be found in his treatise *The evidences of natural and revealed religion*, proposition XIV.

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62. L.L. Whyte (editor), *Roger Joseph Boscovich*, London, Allen and Unwin, 1961.

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64. S. Farr, *A philosophical enquiry into the nature, origin and extent of animal motion*, London, T. Beckett & P.A. D'Hondt, 1771.

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66. *Ibid.*, vol. 5, p.95.

67. *Ibid.*, vol. 5, p.94.

68. *Ibid.*, vol. 5, p.96.

69. *Ibid.*, vol. 5, p.100.

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71. For an account of Reid's life and work, see G. Wilson, *Life of Dr. John Reid*, Edinburgh, Sutherland and Knox, 1852.

Chapter 2. On power in the physiologies of William Cullen and his pupils, John Brown and Benjamin Rush.

One of the most influential of the 18th century Scottish physiologists was William Cullen (1710-1790), who held chairs in chemistry and the institutes and practice of physic first in Glasgow, then in Edinburgh, from 1751 until 1789. As well as being deeply interested in chemistry, (one of his protégés was Joseph Black, who succeeded him to the Glasgow chair), he wrote a seminal work on nosology and constructed an essentially dynamical theory of physiology and pathology which was influential well into the next century.[1] Its influence was partly due to the teachings of two of his students, John Brown and Benjamin Rush, whose own theories, though differing sometimes markedly from his, retained several of its basic concepts.

As T.S. Hall[2] has mentioned, Cullen derived many of his ideas, particularly on the body-soul relation and on power, from slightly earlier ideas that had been developed on the continent; he envisaged, as had Hieronymus Gaubius (1705-1780), Boerhaave's successor at Leyden, and hierarchy of powers in the animal oeconomy: it began with an animal power in the brain, which excited a nervous power in the nerves, which in turn excited a power inherent in muscles, namely the *vis insita* or irritability of Haller.

Cullen willingly acknowledged his debts to men like Boerhaave, Gaubius and Haller; and on the whole, Comrie's assertion[3] that his influence during his own lifetime was largely due to his personal qualities as a physician and teacher, rather than to great originality, seems to be correct.

Nonetheless, Cullen was not simply an eclectic, and power was one theme on which he had original ideas. He appreciated, as did other medical theorists, the philosophical ambiguity in the word 'power'; thus, in discussing the nervous system, though wanting to use the term 'nervous power', he realized that no precise meaning could be given to it and he opted for the term 'nervous fluid' which seemed to be a more useful expression;[4] unlike Boerhaave, he did not envisage anything substantial by it. Cullen considered his greatest problem to be the mechanism of muscle action; in solving that, he used the Gaubian hierarchy of powers in the animal oeconomy, and speculated that the nervous and muscular powers might be correlated at a fundamental level with the animal power; precisely what the animal power was, "whether you consider it as a sentient principle or a mechanical energy", he did not know.[5] In envisaging this fundamental correlation of the vital powers he was disagreeing with Haller, who had insisted on a distinction between the *vis insita* and *vis nervea*; and in seeking a semi-quantitative expression for that correlation he developed a view of the animal oeconomy as a balance between the power put into it and the power that it exerted. Thus, vital power was not *sui generis*, but arose from the energy of an external stimulus impinging on the organism:

> "The force of contraction, or the vigour of muscular fibres, will be always as the force of stimulus and the vigour of the animal, nervous and inherent powers taken together".[6]

Muscle activity always required the energy of a stimulus to activate a type of potential energy residing in the organism, and during such activity the animal oeconomy would lose some of its energy. The consequence of that loss, in Cullen's system, was that a state of rest, such as sleep, was necessary for the restoration of the vital powers.[7] Like a machine, the animal was not a *perpetuum mobile*.

Another aspect of Cullen's dynamical physiology was the topic of 'sympathy' which dealt with the apparent cooperation and sensitivity one to another of the parts of the animal body. Theories of sympathy were usually dynamical ones, and Cullen rejected them because they gave too ready a credence to the reality of powers; such powers as in the theories of Friedrich Hoffmann (1660-1742), Henri Joseph Rega (1690-1754) and Robert Whytt were, in his opinion, 'occult qualities', and as Newton had said, they were to be exorcised. Yet Cullen simply had to use the terminology of power, force and sympathy because of its heuristic value; he wrote of the animal oeconomy essentially as a closed system of powers which were interrelated at a fundamental level, and which could break out of their closed system, namely by doing work and expending themselves, only when stimulated by an external agent or by the mind. The explanatory competence of his theory can be seen in his account of fevers:

> "The remote causes are certain sedative powers applied to the nervous system which, diminishing the energy of the brain, thereby produce a debility in the whole of the functions, and particularly in the action of extreme vessels. Such, however, is at the same time the nature of the animal oeconomy, that this debility proves an indirect stimulus to the sanguiferous system; whence ... the action of the heart and larger arteries is increased and continues so, until it has had the effect of restoring the energy of the brain, of extending this energy to the extreme vessels, of restoring therefore their action, and thereby overcoming the spasm affecting them".[8]

Out of the question of 'excitability' of the vital parts arose a great deal of trouble between Cullen and his former pupil, John Brown. Brown was especially critical of his ideas on spasm and his frequent use of antiphlogistic regimens, (whereby the physician tried to weaken the patient in the hope of removing 'excesses' from his system); in his satirical poem, *The Brunoniad,* Brown depicted his former and kindly teacher in a ghastly light:

> "Say, what's the plan when patients weak and poor
> Nauseate and vomit? Yet to weaken more.
> When the pure stream, by rushing torrents fed,
> Flows copious from the labouring bosom? Bleed.
> If the dread foe uninjured still remain,

What next shall quell the deluge? Bleed again.
What general rules shall fleeting life preserve?
Vomit and purge and bleed and sweat and starve.
... When baleful febris with unhallowed breath,
Breathes on the panting wretch the blast of death,
Ask what sad cause contracts his aspect wan,
... 'Tis spasm, 'tis spasm, th'exulting hero cries,
And rolls in majesty his awful eyes".[9]

That Cullen was not a dogmatic theoretician on fever, spasm or anything else, so far as his clinical work was concerned, has been shown by Guenter Risse in a recent paper.[10] Brown's invective was the product of his own ill temper. One idea, however, on which Cullen stood firm was that in physiology, causes and powers were to be considered to operate solely "from a physical necessity".[11] Hence, the Stahlian ideas on the body-soul relation were dismissed as being incompatible with the useful practice of medicine; and even more significantly, since he owed so much to Gaubius, Cullen declared that

> "... whatever I say in joining with Dr. Gaubius with regard to the soul having a power of beginning motion, yet in matters of physic we must entirely abstract from it".[12]

Power and force were to be studied in so far as they could be investigated on a physical level, and though he wrote sometimes as a fully committed vitalist, Cullen also gave the impression that physiology would have to deal equally with inorganic and organic powers, since both appeared to be indispensable for life. This philosophy was taken up by John Brown.

John Brown (1735-1788)

Brown studied philosophy and theology at Edinburgh, and then medicine. At Edinburgh he was befriended by Cullen and even lived in his home; however, within a few years they fell out and Brown became vituperatively critical of Cullen's work. Indeed, Brown became critical of all medical theories, the only man to receive his praise being Joseph Black for his work in chemistry.[13]

By and large, Brown's theory of physiology was original although it owed something to Cullen's. Both were dynamical theories, but as T.S. Hall has asserted,[14] Cullen's had a more physiological, Brown's a more pathological, orientation. Brown's system is difficult to summarize, for his own explanation of it was often unclear and, although he had a posthumous following on the continent, it was rare for his followers to support his theory in its entirety. Consequently, the following analysis of his system will be more succinct and uncomplicated than Brown himself would wish, although it is worth emphasizing that his followers were impressed by its simplicity.

Brown envisaged the animal oeconomy as a wholly dynamical system. It contained powers which he called 'excitability'; whenever an external power or stimulus struck this system, the excitability would be roused and work or activity would ensue; such activity was called 'excitement'. Health occurred when a suitable balance was kept among the stimuli, excitabilities and excitements. Diseases could arise in two ways: if the stimuli were too strong they would consume too much excitability, too little excitability would remain in the oeconomy and 'sthenic' diseases or diseases of 'indirect debility' would occur; if the stimuli were weaker than usual, excitability would accumulate in the system and 'asthenic' diseases or diseases of 'direct debility' would result. Sthenic diseases were curable with an anti-phlogistic regimen, namely with weak and scanty food. Asthenic ones were curable with a phlogistic regimen, namely with nourishing and abundant food and drink.[15] The novelty and appeal of this scheme was that thitherto in medical practice, physicians had tried generally to weaken the animal system by bleeding, cupping and other evacuant measures, whereas Brown declared that

> "... in all diseases, in which others had supposed there was an abundance of blood, there was a deficiency, that the real causes of these diseases was debility, arising from defect of blood and other stimuli; and that stimulants, given in proportion to the degree of the cause, were the proper remedies".[16]

In fact, he said that 97 out of every 100 ailments were caused by debility, and the physician's duty was immediately to restore the patient's strength. He criticized the debilitating diets usually given to hospital patients, touching a cord of popular sentiment in declaring that such diets killed as many people as did poisons and disease.[17]

Among the principal and sometimes useful features of his theory were the following: 1) His analysis of the concepts of power and force. 2) His attempt to express his theory quantitatively. 3) His attempt to interrelate, even correlate, physico-chemical, mental and organic powers. 4) His emphasis on caring not only for a patient's body, but also for his mind. Taking these in order: what he wrote about power was apparently not obvious to his contemporaries; he felt obliged to emphasize that a force or stimulus acting on the animal oeconomy could have only a positive effect, that it could only cause more work to be done, and never less; thus, there could never be a power which, in acting upon the animal oeconomy, did not have a stimulating effect, since all powers caused expenditure of power when they acted.[18] Some powers were more stimulating than others, and if the mean between stimuli and excitability was disturbed, disease occured. This Platonic doctrine of the mean was an integral part of his attempt to express his theory rigorously; it is best explained in his own words:

> "The accumulation, increase or abundance of excitability, take any term you please, is not occasioned by any action or operation, but by the want of action, the want of operation. To form an adequate idea of it, suppose a scale of excitability of 80 degrees,

as in the line here drawn.

EXCITING POWER

0	10	20	30	40	50	60	70	80
80	70	60	50	40	30	20	10	0

EXCITABILITY

At the commencement of life, the sum total assigned is understood to be 80, because no part as yet is wasted by the action of stimuli. Next, it is wasted in proportion as these are applied from the beginning to the end of the scale. Its wasting is therefore owing to action and operation, but its accumulation to the reverse, the want of action or operation of the exciting powers, as is expressed by the numbers placed above those first mentioned. Thus one degree of exciting power applied takes off one degree of excitability, and every subsequent degree impairs the excitability in a proportion exactly equal to its degree of force. Thus, a degree of stimulant or exciting power equal to 10 reduces the excitability to 70; 20 to 60; 30 to 50; 40 to 40; 50 to 30; 60 to 20; 70 to 10; 80 to 0. And on the contrary, the subtraction of stimulant power allows the excitability to accumulate. Thus, when the excitement is at 79, constituting only 1 degree of life, take off 1 degree of exciting power, and 2 degrees of excitability will arise.... Hence, death takes place from nothing positive, but from the negation of the only means by which life is supported; which are the several exciting or stimulant powers, now fully explained".[19]

Life was therefore proportional to the stimuli acting upon the organism plus the excitability within it; from the tenor of his writing Brown seems to have believed this system not to be a *perpetuum mobile*, and life not to be *sui generis*. To support this interpretation, however, the historian has to discover whence, in Brown's opinion, the power of excitability arose. Unfortunately, Brown never asked that question; the closest he got was to say that excitability needs periodic restoration - during sleep.[20] The mechanism of that restoration was not discussed; and the reason for that omission was probably that he was reluctant to philosophize about causes, particularly those of powers and forces, "as being in general incomprehensible, and as having ever proved the bane of philosophy".[21]

Our problem is perhaps resolvable, for a follower of Brown, a Mr. Christie, did explain what he understood of Brown's theory; and in his account, the powers of excitability came from a source outside of the animal oeconomy, which he compared with a household fire. The grate of the fire represented the human frame; the fuel in the grate represented 'the matter of life', the 'excitability' of Brown's system and the 'sensorial power' of Erasmus Darwin; the shute whereby the fresh fuel is supplied to the fire represented the power of living systems to reproduce excitability; the air denoted the stimuli that act on living systems; and the flame represented life, namely "the product of the exciting powers acting upon the excitability".[22] Christie's summary was

> "As Dr. Brown has described life to be a 'forced state', it is fitly represented by a flame, forcibly drawn forth from fuel little disposed to combustion, by the constant application of streams of air poured into it".[23]

Whether this analogy was faithful to Brown's intention might be debated; but it was approved by Thomas Beddoes (1760-1808) who was Brown's first editor, who thought that Christie had explained admirably the great paradox at the core of the Brunonian system, namely that

> "... food, drink and all the powers applied to the body, though they support life, yet consume it; for he will see that the application of these powers, though it brings forth *life*, yet at the same time it wastes the excitability or *matter of life*, just as the air blown into the fire brings forth more *flame*, but wastes the *fuel* or *matter of fire*".[24]

Thus, Brown regarded the living system just as most natural philosophers regarded mechanical systems - that the power output was always dependent on the input. Although this view must be admitted as akin to a rough idea of the conservation of energy, it was still far from that; for instance, when physiologists wrote about a balance of powers between an animal's intake and its output, they often thought in terms of a material balance too; they rarely drew a rigorous distinction between dynamics and chemistry, and they rarely declared their theories to be wholly dynamical. Brown seems to have held an almost wholly dynamical view of the living system, (which makes Christie's analogy infelicitous), but the impression of his ideas and the complexity of the topic precluded him from sensing that any grand principle of power was in the offing.

Another feature of his system was its quest for simplicity. In seeking this, he proposed that all powers, organic and inorganic, are derived from a common source and are interconvertible. His proposal began with the axiom that a particular cause always has a particular effect, and each particular effect has a particular cause; secondly, since all stimuli which act on living organisms produce one particular effect, life, those stimuli must be fundamentally identical agents; thirdly, denying any inherent power peculiar to living systems, and holding that all living actions are the result of stimuli, he equated organic with inorganic powers.

> "Whatever thing produces the same effect as another, or several other things, must be the same thing as each of them, each of them the same thing as it, and every individual of the whole set the same as every other individual".[25]

And

> "All powers which support any sort of life are the same.... All the powers, therefore, that support any state of life are the same in kind, only varying in degree".[26]

Clearly Brown was using the same maxim as Mayer would some seventy years later, namely *causa aequat effectum*; by defining life as a single, indivisible effect, each cause of it would be logically identical in kind. Mayer, as is well known, contemplated not the qualitative implication of the maxim (which Brown did), but rather its quantitative implication, namely the net conservation of causal agents.

A fourth feature of Brown's system was its concern with the whole person, body and mind, which resulted from the idea that life is the effect solely of the stimuli, whatever their nature, which act on the vital oeconomy; emotions and mental stimuli were as important as physical stimuli, and they were all within the province of the physician.

Despite the impressive features of Brown's physiology, it met stiff resistance, especially in Britain. One reason was probably that given by the historian Charles Daremberg, "*rien de plus simple, et par conséquent rien de plus fausse*";[27] Brown's critics often felt his system was too simple to be true. A second reason was his abraisive personality; one historian has called him "the disputatious and disreputable Brown",[28] and another has called him "a coarse man of low habits".[29] A third reason was that he handled the intricate subject-matter of physiology with insufficient care, so that his critics found it easy to pick holes in his system; thus, Beddoes recounted how John Hunter had felt obliged to correct Brown's confusion between excitement and strength:

> "In the first promulgation of his doctrines, Dr. Brown did not sufficiently distinguish between the actions of the living body and its powers. Excitement and strength were first considered by him as synonymous terms;

> and on the state of excitement, his distinction of diseases was entirely founded.... After many discussions of his doctrine, in which the distinction between the powers and actions of the living body ... was pressed upon him, he adopted the term *excitability* to express the disposition to action, and to replace the terms *irritability*, *sensibility* and *inability*, which he had discarded from his system".[30]

According to the *Dictionary of national biography*, the useful features of Brown's dynamical system passed eventually into common medical theory[31] and his system as such faded away by c.1820. One lasting influence it did exert was to insist so strongly on the total dependance of life upon outside forces. The great paradox of those forces was that, though they formed and maintained life, they also produced its dissolution; as Brown himself wrote,

> "... life, the prolongation of life, its decay and death, are all [dynamical] states equally natural".[32]

Benjamin Rush (1745-1813)

As well as being the most eminent physician in North America during the late 18th and first decade of the 19th century, Rush was one of its most influential social reformers and an energetic pamphleteer. He was a signatory of the Declaration of Independence, a close friend of Tom Paine,[33] and from 1799 until his death was treasurer of the national mint. His academic career was equally impressive: having begun his medical education at the College of Philadelphia, he was sent to Scotland to study under William Cullen and Joseph Black, and then to London where he spent a while with William (1718-1783) and John (1728-1793) Hunter. Meanwhile, he had been assured that on his return to Philadelphia he would be made the first incumbent of the chair of chemistry; this duly happened, and when in 1791 the college became a university, he was made its first professor of the Institutesand Practice of Medicine.

A sizeable number of papers have been written by American medical historians on Benjamin Rush. They have discussed his education,[34] especially under Black in Glasgow and Cullen in Edinburgh where, of course, he came under the influence of Boerhaaveian chemistry; his academic career;[35] his work as a physician, especially during the Philadelphia yellow fever epidemic of 1793;[36] and his work as a psychiatrist,[37] with which his own last publication, *Medical inquiries and observations upon diseases of the mind*, dealt. Some of these papers discussed the similarities between Rush's physiology and the systems of Cullen and John Brown, but none, to my knowledge, have focussed on his concept of power vis-à-vis the animal mechanism.

Of the two - Cullen and Brown - the latter seems to have made his mark more strongly on Rush's physiology even though, as Shryock has pointed out,[38] Rush strongly denied that Brown had been his chief guide and mentor. The similarities between their systems, however, were just as obvious to Rush's American contemporaries as they are to us today: for instance, his physiology was largely dynamic, and the key ideas of stimulus, excitability and excitement were used in exactly the same sense for the animal oeconomy as Brown had used them. (Indeed, one might even call his physiology wholly dynamic, for, although he enthusiastically advocated the application of chemistry to physiology, whenever it came down to fundamental theorizing, the paradigms he used were stimulus, excitability and power and rarely chemical terminology. This point has been too rarely appreciated by students of his work). Rush put into practice Brown's emphasis on treating the whole person - body and mind; thus, he was thus one of the earliest American physicians to devise occupational therapies for the mentally ill and to encourage analytical conversation with such patients. He developed especially Brown's idea that life is proportional to, and always dependent on, the stimuli impinging upon the organism: indeed, he stated explicitly that the living organism is a mechanism whose energy output absolutely depends on its energy intake, that organic power is only a modification of inorganic power, and that a *perpetuum mobile* cannot exist either in life or in Nature. We find these ideas expressed most succinctly in a lecture of 1799:

> "Life is the EFFECT of certain stimuli acting on the sensibility and excitability, which are extended in different degrees over every external and internal part of the body. These stimuli are as necessary to its existence, as air is to flame. Animal life is truly (to use the words of Dr. Brown) 'a forced state'. I have said the *words* of Dr. Brown; for the opinion

was delivered by Dr. Cullen in the University of Edinburgh in the year 1766, and was detailed by me in this school many years before the name of Dr. Brown was known as a teacher of medicine. It is true Dr. Cullen afterwards deserted it; but it is equally true, I never did; and the belief in it has been the foundation of many of the principles and modes of practice in medicine which I have since adopted. In a lecture which I delivered in the year 1771, I find the following words, which are taken from a manuscript copy of lectures given by Dr. Cullen on the institutes of medicine: 'The human body is not an automaton, or self-moving machine; but is kept alive and in motion by the constant action of stimuli upon it.' In thus ascribing the discovery of the cause of life, which I shall endeavour to establish, to Dr. Cullen, let it not be supposed that I mean to detract from the genius and merit of Dr. Brown. To his intrepidity in reviving and propagating it, as well as the many other truths contained in his system of medicine, posterity, I have no doubt, will do him ample justice, after the errors that are blended with them have been corrected..."[39]

As had Cullen and Brown, Rush discussed two types of stimuli - external and internal - as the primary causes of life. On external stimuli, he found especial delight in a saying of Antoine Lavoisier (1743-1794) which was to be quoted frequently by British and American physiologists in the first half of the 19th century. So powerfully did this saying impress physiologists that it is worth quoting:

> "Organization, sensation, spontaneous motion and life exist only at the surface of the earth, and in places exposed to *light*. We might affirm that the flame of Prometheus's torch was the expression of a philosophical truth that did not escape the ancients. Without light, Nature was lifeless, inanimate and dead. A benevolent God, by producing life, has spread organization, sensation and thought over the surface of the earth".[40]

If any physical agent was thought of as the primary cause of life, it was light; in this, Lavoisier merely articulated a belief of many a physiologist. But whereas Lavoisier almost certainly envisaged this light-life relation as a material one, Rush seems to have had an essentially dynamical dependence in view: thus, he wrote of life being the effect of impressions upon peculiar species of matter, "as sound is of the stroke of a hammer upon a bell, or music, the motion of the bow upon the strings of a violin".[41] Hence, he proscribed the various entelechies of Whytt, Stahl and others, the *vis medicatrix* of Cullen and the vital principle of John Hunter. For him, life was purely the effect of stimuli and it was "as truly mechanical as the movements of a clock from the pressure of its weights".[42]

Rush rounded off his ideas on life and power with a theological exposition. Of course, this is not surprising if one bears in mind the tradition of discussing power in a theological context (which Chapter 1 discusses), and Rush's own deeply religious background. His dynamical physiology harmonized perfectly with his belief in God the

Father and the Son as the only beings capable of self-existence and self-motion. St. John had written

> "For as the Father hath life in himself, so hath he given to the Son to have life within himself".[43]

And Rush wrote

> "To suppose a principle to reside necessarily and constantly in the human body, which acted independently of external circumstances, is to ascribe to it an attribute which I shall not connect, even in language, with the creature man. Self-existence belongs only to God....
>
> The doctrine I have taught cuts the sinew of this error; for, by rendering the continuance of animal life, no less than its commencement, on the effect of the constant operation of divine power and goodness, it leads us to believe that the whole creation is supported in the same manner".[44]

Summary

In the physiologies of William Cullen and two of his eminent pupils, John Brown and Benjamin Rush, there was an essentially dynamical view of the living oeconomy. From this exclusive interest in the powers of the organism, vis-à-vis the powers of Nature, they enunciated the important ideas of 1) A fundamental interrelation between organic and inorganic powers, which Brown and Rush envisaged as an actual correlation; and 2) The living oeconomy as a piece of mechanism, whose expenditure or manifestation of power is strictly proportional to its power intake.

It can be argued that Cullen, having held these ideas, rescinded them by introducing his *vis medicatrix naturae*. Nonetheless, Brown and Rush inherited them from him; and although Brown only implicitly developed them into the idea that the organism never creates its own power, Rush made this idea explicit. Strictly speaking, neither of them enunciated a full-blooded principle of conservation of power; they never asked whether power is lost or destroyed in its transmission through the living oeconomy; but they surely saw no need to ask that question, since their aim was only to show that life cannot be *sui generis* and cannot entail the creation of power. Of the two components of conservation of power - namely, its non-creatibility and indestructibility, it was the former that Brown, Rush, and maybe Cullen, propounded in their physiological theorizings.

NOTES TO CHAPTER II.

1. All extracts in this chapter are from J. Thompson (editor), *The works of William Cullen*, 2 vols., Edinburgh, W. Blackwood, 1827.
2. T.S. Hall, *Ideas on life and matter*, Chicago, University of Chicago Press, 1969, Vol.2, p.93.
3. J.D. Comrie, *History of Scottish medicine*, London, The Wellcome Historical Medical Museum, 1931, Vol.1, p.303.
4. W. Cullen, *Institutions of medicine*, in *op.cit.*, Note 1 above, Vol.1, p.17.
5. *Ibid.*, pp.67-68.
6. *Ibid.*, p.83.
7. *Ibid.*, p.136.
8. W. Cullen, *First lines of the practice of physic*, *op.cit.*, Note 1 above, Vol.1, p.501.
9. J. Brown, *The Brunoniad, a poem in six cantos*, London, G. Kearsley, 1789, p.9.
10. G.B. Risse, 'Doctor William Cullen, physician, Edinburgh', *Bull.Hist.Med.*, 1974, 48:338-351.
11. *Op.cit.*, Note 4 above, p.23.
12. *Ibid.*, p.23.
13. *Op.cit.*, Note 9 above, pp.22-23.
14. *Op.cit.*, Note 2 above, p.91.

15. J. Brown, *The elements of medicine*, ed. Thomas Beddoes, London, J. Johnson, 1795, Vol.1, pp.xv-xix.

16. *Ibid.*, Vol.1, p.xxiv.

17. *Ibid.*, Vol.1, p.lxxviii.

18. *Ibid.*, Vol.1, p.8.

19. *Ibid.*, Vol.1, pp.25-26.

20. *Ibid.*, Vol.2, pp.325-326.

21. *Ibid.*, Vol.1, p.8.

22. *Ibid.*, Vol.1, p.cxxx.

23. *Ibid.*, Vol.1, p.cxxx.

24. *Ibid.*, Vol.1, p.cxxxi.

25. *Ibid.*, Vol.1, p.cxxxi.

26. *Ibid.*, Vol.2, p.33.

27. *Op.cit.*, Note 2 above, p.97.

28. T.C. Allbutt, cited in F.H. Garrison, *An introduction to the history of medicine*, 4th ed., London, W.B. Saunders Company, 1929, p.314.

29. *Ibid.*, p.314.

30. *Op.cit.*, Note 15 above, Vol.1, p.cxlvii.

31. *Dictionary of national biography*, London, Smith, Elder & Co., 1909, Vol.III, pp.14-17.

32. *Op.cit.*, Note 15 above, Vol.2, p.43.

33. Rush and Thomas Paine were close friends and were among the most ardent revolutionaries in America before and during the War of Independence. Rush

suggested the title for Paine's historic pamphlet 'Common Sense'.

34. H.S. Klickstein, 'A short history of the professorship of chemistry in the university of Pennsylvania school of medicine, 1765-1847', *Bull.Hist.Med.*, 1953, 27:43-68.

35. *Ibid.*, and R.H. Shryock, 'The medical reputation of Benjamin Rush', *Bull.Hist.Med.*, 1971, 45:507-552.

36. S. Jarcho, 'John Mitchell, Benjamin Rush and yellow fever', *Bull.Hist.Med.*, 1957, 31:132-136. And L.H. Butterfield, 'Benjamin Rush: a physician as seen in his letters', *Bull.Hist.Med.*, 1946, 20:138-156.

37. F. Wittels, 'The contribution of Benjamin Rush to psychiatry', *Bull.Hist.Med.*, 1946, 20:157-166.

38. *Op.cit.*, Note 35 above.

39. B. Rush, 'Lectures on animal life', 1799, in D.D. Runes (editor), *The selected writings of Benjamin Rush*, New York, The Philosophical Library, 1947, pp.136-137.

40. *Ibid.*, p.138.

41. *Ibid.*, p.174.

42. *Ibid.*, p.175.

43. The gospel according to St. John V., verse 26.

44. *Op.cit.*, Note 39 above, p.179.

Chapter 3. On Power and Causality in the physiologies of Erasmus Darwin, Samuel Farr and Gilbert Blane

Erasmus Darwin (1731-1802) is one of those figures in the history of science whose work will be open always to very varied assessment. Few historians today would call him a major figure in the development of British science, yet several of his learned contemporaries regarded him most highly, and he was a founder member and driving spirit of the Lunar Society of Birmingham. Samuel Taylor Coleridge, the poet, despite his dislike of Darwin, admitted that he possessed "perhaps a greater range of knowledge than any other man in Europe, and is the most inventive of philosophical men".

The differences among historians' assessments of Darwin's work in natural philosophy arise largely from his being "the most inventive of philosophical men". On the one hand, he has been dismissed as a mere visionary, whilst on the other hand, he has been likened to Leonardo da Vinci and Goethe for his excellence in both practical and theoretical work in a wide range of topics.[1] It seems to me that a balance between these extremes is most likely to be valid.

Darwin was much quoted by late 18th and early 19th century natural philosophers and especially by naturalists and physiologists. Like John Brown, whose ideas his own often resembled, he was admired by some, ridiculed by others, and praised by many for the penetration of some of his ideas. Although his speculative turn of mind and poetic turn of phrase will always seem alien to the scientific spirit, there were and always will be reasons to take his scientific ideas seriously. Firstly, he was undoubtedly recognized as one of the finest medical practitioners of his day. Secondly, his wide reading in medicine and natural philosophy, his willingness to quote and acknowledge the work of other men, his perceptiveness and his ability to ask really stimulating questions gave his writings considerable authority and kept his ideas generally within the respectable currents of natural philosophy.

Darwin's belief in a wholly dynamical physiology was one instance of his agreement with certain other physiologists, which was supported by fairly sober theorizing and empirical evidence and impressed later workers. His ideas on power and causality were, however, intricate and were often presented alongside other ideas. For instance, in *Zoonomia* he discussed the origin of ideas alongside the question of the origin of animal motion; and his solution of the former - namely, that ideas cannot be created *de novo* - was presented as a parallel to his solution of the latter - namely, that animal motion cannot arise *de novo*. This parallel between mind and body was a basic feature of his physiology and earned him the reputation of a materialist,[2] since the agent which moved the body, which he called 'the spirit of animation', was of the same genre as heat, electricity, magnetism and allied agents.

Darwin enumerated six fundamental laws of animal oeconomy, in which we can see his closeness to, and departure from, the physiologies that were being developed by Cullen and Brown.

> "1) The fibres which constitute the muscles and organs of sense possess a power of contraction ....
>
> 2) The spirit of animation is the immediate cause of the contraction of animal fibres, it resides in the brain and nerves, and is liable to general or partial diminution or accumulation.
>
> 3) The stimulus of bodies external to the moving organ is the remote cause of the contractions of animal fibres.
>
> 4) A quantity of stimulus produces irritation, which is an exertion of the spirit of animation exciting the fibres into contraction.
>
> 5) A certain quantity of contraction produces pleasure; a greater or less quantity, if it be perceived at all, produces pain; these constitute sensation.
>
> 6) A certain quantity of sensation produces desire or aversion; these constitute volition".[3]

Darwin sought a semi-quantitative connection between vital actions and the external stimuli on which they depended; thus, all muscular activity was supposed to be strictly proportional to "the energy of the sensation that excites them and the quantity of sensorial power".[4] Similarly for muscular motion arising from volition. His theory of health rested on a balance or crasis between stimuli and sensorial powers (or the spirit of animation) on the one hand, and vital activity on the other; excessive activity would exhaust the power of the vital oeconomy, whilst rest, respiration and food were required to replenish that power;[5] the vital oeconomy was therefore no *perpetuum mobile*.

One ambiguity in Darwin's theory is what he meant by 'power' and 'spirit'. Several early 19th century commentators thought that he meant something substantial and material, akin to Newton's 'subtile aether', and that power and force in the wholly dynamic sense were no part of his scheme. He himself gave good grounds for this interpretation, asserting that the spirit of animation 'resided' in the brain and nerves and was 'secreted' from them. Yet such terminology was also used by Cullen, as we have seen, even though he would not commit himself to any theory on the nature of the *vis nervea*. Darwin was equivocal about power and spirit simply because he knew how indefinable such terms were; a reply he gave to someone who urged him to become a Christian reveals his, and his contemporaries', difficulties with power:

> "Before I do that, you Christians must all be agreed. The other morning I received two parcels; one containing a work of Dr. Priestley's proving there is no spirit; the other a work by Berkeley, Bishop of Cloyne, proving there is no matter. What am I to believe amongst you all?"[6]

That Darwin tended greatly towards a dynamical philosophy can be seen in a discussion on causality at the close of volume 1 of *Zoonomia*. This discussion is also notable since it was a rare instance of his use of theology to bolster a scientific argument, (Darwin was not a Christian, but neither was he an atheist as some 19th century philosophers asserted), and because it proposed something implicit in earlier sections of *Zoonomia*, namely that an effect could never be greater than its cause.

> "These causes may be conveniently divided into two kinds, efficient and inert causes, according to the two kinds of entity supposed to exist in the natural world, which may be termed matter or spirit .... The efficient causes of motion or new configuration consist either of the principle of general gravitation, which actuates the sun and planets; or of the principle of particular gravitation, as in electricity, magnetism, heat; or of the principle of chemical affinity, as in combustion, fermentation, combination; or of the principle of organic life, as in the contraction of vegetable and animal fibres.
>
> This perpetual chain of causes and effects, whose first link is rivetted to the throne of God, divides itself into innumerable diverging branches which, like the nerves arising from the brain, permeate the most minute and most remote extremeties of the system, diffusing motion and sensation to the whole. As every cause is superior in power to the effect which it has produced, so our idea of the power of the Almighty Creator becomes more elevated and sublime as we trace the operations of nature from cause to cause, climbing up the links of these chains of being, till we ascend to the Great Source of all things".[7]

Thus, Darwin adhered to the powerful tradition, which was supported by the 18th century philosophers (discussed in Chapter 1), that the powers of Nature are ultimately one with the power of God and that they cannot be created, for creation belongs to God alone; all activities of power are manifestations of His Will.

Darwin's greatest departure from contemporary physiology was his application of dynamics to a detailed study of plants; and despite his polemic ideas in that field, for instance that plants could think and possess passions, his assertion that physiologists should test their ideas in botany as well as in the animal oeconomy was not lost on early 19th century physiologists who, when wishing to justify their comparative approach to physiology, often cited him.

There were other physiologists who, like Darwin, had a dynamical view of the organism, were much cited during the late 18th or early 19th century, and were clearly intent on discovering a comprehensive theory of life. They often freely used speculation instead of experimentation, and for that reason they were quite forgotten by the mid 1800s, although their systems were well known and regarded in their own day. Only two of those medical theorists shall be discussed: Samuel Farr (1741-1795) and the eminent physician Gilbert Blane (1749-1834).

Farr had studied medicine at Edinburgh, graduated at Leyden, and served as a physician in the Bristol Infirmary from 1767 until 1780. He published several medical works that were highly esteemed; most dealt with the problems he faced as a working physician, but his second treatise was a curious, wholly philosophic or speculative venture into animal physiology, of the same genre as the treatises of the early 18th century overtly Newtonian physicians like

Archibald Pitcairne, George Cheyne and John Freind. Farr's treatise was *A philosophical enquiry into the nature, origin and extent of animal motion, deduced from the principles of analogy and reason*,[8] first published in 1771. He took his cue from Newton, especially from the 'Quaeries' which he thought had not been sufficiently respected by medical theorists. He aimed to construct a theory whereby the powers and motions of the inorganic and organic worlds could be reduced to a single power, namely repulsion; repulsion was supposed to be the original state of Nature, and upon it all the laws of gravitation and attraction depended.[9] This universal principle of repulsion and its resultant motions were further reduced to being the effects of the mind, and the powers of the mind were attributed to God as the primary cause of all power.[10] Several features of the theory were traditional and accorded with contemporary natural philosophy and theology; Farr's originality lay in his enunciating such ideas more boldly and eloquently than most fellow physicians cared to do. Moreover, although his theorizing was sometimes excessively speculative, it was difficult to disagree with the general tenor of his analysis.

Farr argued that many somatic diseases originate in mental disorders. He also implied that there was a rigorous connection between the powers of the mind and those of the body, citing reputable authorities like Hippocrates, Whytt and Cullen. Indeed, as most physicians were generally agreed that mental conditions could powerfully influence the body, they were well disposed to his assertion that, since passions are dynamical agents, major ailments could be caused by them. Farr mentioned cancers, rheumatism, gout, phthisis ~~of the lungs~~, hydrophobia, apoplexy and palsy among such psycho-somatic disorders;[11] and as Pedro Laín-Entralgo's monograph[12] on the mind-body problem has shown, there would have been strong philosophical and religious reasons, leaving aside the physiological ones, for Farr's contemporaries to give such ideas a hearing.

His ideas on animal functions, particularly those concerning the power of the mind over the body, led into a dynamical theory of opposites: that attraction not only depends upon repulsion, but that they actually necessitate each other; otherwise there would be universal and never-ending activity, and activity without rest would be as deleterious for the world as never-ending stagnation. Farr therefore proposed a fundamental principle or power of 'universal activity', whose basic physical manifestation was repulsion, yet which could also manifest itself as attraction.[13] One of his assumptions, though never explicated, was that power could never be created since it derived ultimately from God, via the mind. Therefore, there was no place for vital powers; the nature of animal motion was a consumption of the 'universal activity', interspersed with periods of rest during which that activity could be restored by the agency of food.[14]

Farr's ideas often resembled Boscovich's, Cullen's and Brown's, although he was extremely reluctant to admit any indebtedness. His contemporaries realized how well his theory fitted current trends in natural philosophy and physiology, and they referred to him not infrequently as 'ingenious'; some of them even accorded him as much attention as they did to Erasmus Darwin, and to some extent he merited as much, for his practical medical treatises were excellent and showed how speculative physiology could be reconciled with sound practice.

Similar ideas to Farr's were expounded in a lecture delivered in 1788 by *Gilbert Blane*. Blane had studied and graduated in Edinburgh, and through Cullen's recommendation became associated with William Hunter in London. In the 1780s he served as a physician to the British fleet in the West Indies, becoming head of the Navy Medical Board in 1795. In the 1790s he was responsible for the compulsory use of lemon juice throughout the British navy, and in recognition of his work was elected to several learned societies, was knighted and became George IV's physician. Among his several published works, two are relevant to this thesis: his *Lecture on muscular motion*,[15] read to the Royal Society of London in 1788 and published as a book in that year, and his oft-cited *Elements of medical logick*[16] [*sic*], 1819.

The lecture is a fine example of how a physician, in studying the mechanism of muscle action, felt obliged to study the nature of motion generally and even the nature of matter. Being unable to discover absolute rest anywhere in Nature, Blane's primary axiom was therefore that motion is an original and natural property of all matter. The causes of motion, namely attraction and repulsion, were to be considered a single simple agent:

> "And it is so universal an agent in nature, that some modern philosophers have made it absorb, as it were, every other power and property of matter. The late Father Boscovich of Milan, about forty years ago, advanced a very bold doctrine to this effect, alledging with great strength of argument, illustrated by geometrical reasoning, that there does not exist in nature any such thing as impenetrable, extended particles.... Whether this hypothesis is founded in truth or not, it would appear from the reasonings made use of, that all the relative properties of matter may be accounted for, though we abstract from every other consideration but attraction and repulsion".[17]

This scheme of inorganic powers gave him a model for the organic ones. Thus, he proposed a wholly dynamical view not only of muscle action, but of the animal oeconomy generally, in accordance with Boscovich's 'bold doctrine'. He even proposed a fundamental relation between organic and inorganic powers.

> "It would appear, therefore, that there is a coordinance or preestablished harmony, as it were, between the faculties of animals and the laws of external matter, which is the foundation of all the instinctive habits of animals, as well as the rational conduct of man; and it is impossible sufficiently to admire that sublime contrivance by which the frame of animated beings is thus in all points adapted to the constitution of inanimate nature".[18]

This view was not of a strict correlation between organic and inorganic powers, but Blane believed it was an important and somewhat original idea of the universal fabric of God, Man and Nature. In a republication of the lecture in 1822, Blane added a philosophical footnote to this passage, in which he emphasized how Leibniz's view of the mind-body relation differed radically from the view that the British philosophers of the 18th century had constructed. The latter had proposed a fundamental unity of the mental and bodily powers, and though Blane did not explicitly declare it, his view was almost identical to theirs. Of his own view, he wrote in the footnote that

> "The learned reader need not be told that the author here refers to the popular doctrine of Leibniz, and that he means merely to allude to it figuratively, without approving or adopting it. This celebrated philosopher held that the obedience of muscles to the will is not to be ascribed to any physical connection between the mind and body; but that the deity has so preordained it that the actions of the mind and body should proceed by a parallel but independent series of movements, like two distinct machines, which without any mutual agency are so constructed as to correspond simultaneously in their motions".[19]

Why did Blane add this footnote thirty-four years after the lecture was first read? One likely reason was that in the meanwhile he had become deeply impressed by Dugald Stewart who had tackled the question of the mind-body relation vis-à-vis their powers, in the Locke-Reid tradition. In 1819, Blane had dedicated his most philosophical medical treatise, *Elements of medical logick,* to Stewart, admitting his intellectual debt to "particularly Bacon, Locke, Reid, and above all, Professor Dugald Stewart, the most profound metaphysician, as well as one of the most elegant writers of this age".[20]

Whereas in his 1788 lecture, Blane had not delved deeper into the causes of animal motion than merely proposing a general attractive power for which no further cause could be discovered, in his *Elements* he intended a deeper investigation. However, so intricate was the problem that he believed it could be solved solely by a thoroughly inductive, philosophical approach, with scarcely any room for experimentation; he began with a defence of causality as a *sine quâ non* for all philosophy and for the practice of physick.

> "The sound state of the mind in Physick, as well as in all other practical pursuits in life, must consist in conceiving clearly and correctly the reciprocal relations of cause and effect; for it is upon such knowledge alone, that the adaptation of means to ends, in which we have defined art to consist, can be founded...."[21]

So universally valid was causality, that he developed his 1788 conception of a coordinance or pre-established harmony between organic and inorganic powers into a firm theory - namely, that since causality appears to be a law both of Nature and the human mind, it indicates that

> "... not only every organ and function of the body, but every faculty of the mind, is co-relative with, or represents and reflects, as it were, the elements and laws of universal nature".[22]

For Blane, the truths of causality and of a correlation between the elements (i.e. the dynamical agencies) of mind and Nature, guaranteed the reality of power. In Lockean vein he wrote

> "We become assured of the reality of such agencies by finding that, in imitating the sequences of nature, we can adapt means to ends, so as to bring about certain proposed results. It seems to be that in this way we acquire our first idea of POWER".[23]

Apparently, he had given that explanation of men's idea of power as early as 1771, in a discourse designed to refute Hume's doctrines, particularly that of custom being the sole source of our idea of causality.[24]

The next step in his *Elements* was to group physiological phenomena into categories, with a specific dynamical agent as the cause or power for each category. There were nine categories of vital energy, which were supposed to be the ultimate facts of physiology and pathology.[25] They were the generative, conservative, temperative, assimilative, formative, restorative, motive, sensitive and sympathetic. This system was considered by most contemporaries as an idiosyncracy and too hypothetical to be useful. Nonetheless, it did fit into the general interest in dynamical physiology of that time and did have some similarity with the systems of Cullen, Brown and John Hunter. What was idiosyncratic was its firm denial that the vital energies might be reduced to a simpler hypothesis containing only one or two energies; it seems as if, having toyed with the Boscovichean theory in his early years, Blane decided later that such simplicity could never characterize the animal oeconomy, and that a string of galenic-type faculties was nearer the mark. Hence he disagreed with Brown.

> "The errors of this ingenious man seem to have consisted in his having erected a system on the narrow foundation of only one of the principles of the animal oeconomy, and in pushing that to an extreme".[26]

Clearly, Blane may not be called an early contributor to the conservation of energy. Nonetheless, he is an excellent example of a physician who felt compelled to examine the ideas of causality and power in order to form his own physiological ideas. His defence of causality and power was to be cited approvingly by physiologists like William Carpenter and Peter Roget who needed to assert that these are real, in order to develop their own dynamical physiology. Indeed, what Reid and Stewart did to re-instate causality and power in pure philosophy, Blane did within the aegis of physiology. In the first chapter of his *Elements*, power was made the central doctrine of physiology. Consequently, that chapter was cited often long after his ideas on the nine vital energies had fallen into oblivion.

NOTES TO CHAPTER 3.

1. D. King-Hele, *The essential writings of Erasmus Darwin*, London, MacGibbon & Kee, 1968, p.11.

2. J. Dawson, *Erasmus Darwin, philosopher, poet and physician*, London, H.K. Lewis, 1861, p.9.
Another useful biography is that written by Ernst Krause, *Erasmus Darwin*, translated from the German by W.S. Dallas, with a preliminary notice by Charles Darwin, London, J. Murray, 1879.

3. E. Darwin, *Zoonomia, or the laws of organic life*, London, J. Johnson, 1794-96, Volume 1, pp.30-31.

4. *Ibid.*, p.43.

5. *Ibid.*, p.72.

6. *Op.cit.*, Note 2 above, p.48.

7. *Op.cit.*, Note 3 above, pp.530-533.

8. S. Farr, *A philosophical enquiry into the nature, origin and extent of animal motion, deduced from the principles of analogy and reason*, London, T. Becket & P.A. D'Hondt, 1771.

9. *Ibid.*, p.59.

10. *Ibid.*, p.119.

11. *Ibid.*, pp.388-389.

12. P. Lain-Entralgo, *Mind and body*, London, The Harvill Press, 1955. Although Lain-Entralgo does not discuss Farr himself, the ideas contained in his *Mind and body* have furnished me with useful guide-lines throughout this thesis.

13. *Op.cit.*, Note 8 above, pp.131-132.

14. *Ibid.*, p.132.

15. G. Blane, *A lecture on muscular motion*, London, J. Cooper, 1788.

16. Sir G. Blane, *Elements of medical logick*, London, T. & G. Underwood, 1819.

17. *Op.cit.*, Note 15 above, p.7.

18. *Ibid.*, p.41.

19. Sir G. Blane, *Select dissertations on several subjects of medical science*, London, T. & G. Underwood, 1822, p.267.

20. *Op.cit.*, Note 16 above, p.19.

21. *Ibid.*, p.15.

22. *Ibid.*, p.18.

23. *Ibid.*, p.19.

24. *Ibid.*, p.19.

25. *Ibid.*, p.22.

26. *Ibid.*, pp.35-36.

Chapter 4. On power in the works of James Hutton, John Playfair and Humphrey Davy

This chapter examines three natural philosophers who were immensely influential in the late 18th or early 19th century. None of them is now regarded as having contributed significantly to physiology, although Hutton's discussions of light, heat and respiration, and Davy's earliest publication on the same topics and on the chemistry of life, related to current problems in physiology and were cited by physiologists. These three men appear in the same chapter because their ideas on the centrality of power or force in natural philosophy were very similar; indeed, as Charles Gillispie has said,[1] Playfair was Hutton's Huxley in championing his theory of the earth; Davy also derived many of his ideas during his early years from Hutton, and for several years after his appointment at the Royal Institution he lectured on geology.

It will be argued that all three contributed significantly to the emergence of the conservation of energy. In Hutton's case, the argument will also be that his ideas on power and in geology owed much to his medical background, and that though he hardly ever practised as a physician, his outlook on the physical world revealed a mind that had been trained to think about vital activity, and whose thought-patterns were essentially those of a physiologist. The only historian who, to my knowledge, has proposed this interpretation of Hutton's world-view has been François Ellenberger.[2]

James Hutton (1726-1795)

Hutton studied humanities and then medicine at Edinburgh, eventually taking his doctorate at Leyden. For twenty years he devoted himself to farming and in 1768 returned to Edinburgh "... giving his undivided attention from that time to scientific pursuits".[3] Though he never acquired an academic position, he became a member of the cool, dispassionate circle of Edinburgh philosophers - men like Joseph Black, Dugald Stewart, Adam Smith (1723-1790), James Watt (1736-1819) and John Robinson (1739-1805) - who were the life-blood of the 'Scottish Enlightenment'. He quickly acquired a reputation as a chemist and, slightly later, as an enquirer into the history and structure of the Earth. He published a handful of works in philosophy and natural philosophy, some of these appearing in the *Transactions of the Royal Society of Edinburgh*, of which he was a member.

Hutton's doctoral dissertation *Dissertatio physico-medica inaugualis de sanguine et circulatione microcosmi*[4] contained ideas which became fundamental to his natural philosophy and geology. Particularly important were:
1) His belief in the validity of final causes in natural philosophy. This category of cause was still thoroughly respectable in the life-sciences, and Hutton's belief in its universal validity was vindicated for him by its clear rôle in vital phenomena. For instance, he was to write in his *Theory of the earth* that

> "The circulation of the blood is the
> efficient cause of life; but life is the
> final cause, not only for the circulation
> of the blood, but for the revolution of the

> globe: without a central luminary and a revolution for the planetary body, there could not have been a living creature upon the face of this earth".[5]

2) His belief in cycles throughout Nature. A large proportion of his dissertation discussed organic cycles, namely the perpetual circulation of the blood and humours,[6] the circulations of nutrition,[7] secretion[8] and digestion,[9] and the exchanges which occur between the three realms of Nature - mineral, vegetable and animal - which constitute a circulation in the macrocosm.[10] Indeed, the dominant theme of his dissertation was that the cycles within the microcosm of the vital oeconomy resemble and relate to the cycles in Nature at large.[11] Of course, this was not a new idea, but Hutton moulded it after his own fashion to lie at the very foundation of his geology.

3) His belief not only in cycles, but in perpetual and in a sense self-sustaining cycles. Thus, despite his aim to treat the organism mechanistically in the spirit of Boerhaave, whose influence on his dissertation is obvious,[12] he departed from a rigorous mechanistic explanation of life, declaring that

> "... the blood and its humours present us with a great vital circle, a very fine example of a *perpetuum mobile*, matter which moves itself without a material cause, procuring for itself - on the fertile earth whatever its needs require, and restoring its decline each day, even as an effect of its own destructive cause".[13]

4) His view of the earth as an organism, and that natural philosophy should employ the concepts and method of the physiologist. His dissertation dwelt upon the perpetual cycle of the elements, uniting the organic and inorganic realms; vegetables nourish themselves on the mineral world, animals feed on vegetables, and by putrefaction the animals reconstitute the mineral world.[14] Just as life was obviously a set of processes, indeed of perpetual cycles, so too was Nature or the macrocosm seen as a dynamic system and ever active.

The following analysis of Hutton's post-dissertation writings will consider two themes: the concepts of organism and power in his geology; and their rôle in his treatises on light, heat and allied topics. Inevitably, organism and power were complementary ideas; the former facilitated a dynamic view of Nature, and in constructing that view Hutton expounded a theory of matter in which natural phenomena, and even substance itself, depended solely on the interactions of a general force of attraction and a general force of repulsion; that theory resembled Boscovich's and if we believe Dugald Stewart,[15] Hutton wanted to reconcile Boscovichean physics with Berkleyan metaphysics.

There were three publications of Hutton's geological theory during his lifetime. In 1785 he read a paper before the Royal Society of Edinburgh;[16] this was printed as an abstract of thirty-two pages for private circulation. There he asserted that the earth is a 'regular' system, in which matter is thrust up continuously from the bowels of the earth by "... the active power of fire and the expansive force of heat", that land is eroded continuously, and that this cycle, so far as human observation is concerned, "... has neither a beginning nor an end".[17] He did not analogize that system explicitly with an organism, (which he did in his later accounts), but life was upheld as the final cause of the entire scheme:

> "An endeavour is then made to support the theory by an argument of a moral nature, drawn from the consideration of a final cause ... and an argument is formed, upon the supposed wisdom of nature, for the justness of a theory in which perfect order is to be perceived. For, according to the theory, a soil adapted to the growth of plants is necessarily prepared and carefully preserved; and in the necessary waste of land which is inhabited, the foundation is laid for future continents, in order to support the system of this living world".[18]

In 1788 Hutton enunciated his theory a second time; in the *Transactions of the Royal Society of Edinburgh* of that year, nearly a hundred pages were given to it.[19] He began with the reasons for a dynamic, ever active, view of the earth, namely that where so many living creatures ply their respective powers in pursuing the ends for which they were made, Nature could hardly ever be quiescent, and matter itself must always be in motion. A fine illustration of the earth ever active was the rôle of the atmosphere: it was a necessary condition for the sustenance of fire, the breath of life to animals, an instrument in vegetation, a giver of fertility and health and a protection against the noxious effects of putrefaction. It was also "the proper means of circulation" for the earth, by raising up the water of the oceans, and releasing water over dry land. That was the mechanism of the earth.

He considered next "some of those powers by which motion is produced, and activity procured to the mere machine".[20] The two most immediately effective powers were light or heat, and cold or condensation. Other powers were required to modify those two primary ones "in the oeconomy of life and system of our changing things".[21] His beliefs in the paramounce of power and the living nature of the earth, whereby its form and essence remain unchanging, led him to ask what happens to power during the earth's cycles.

> "Has the globe within it such an active power as fits it for the renovation of that part of its constitition which may be subject to decay? Are those powerful operations of fire or subterraneous heat ... to be considered as always having been? Are they to be concluded as proper to every part of the globe, and as continual in the system of this earth?"[22]

Clearly, to maintain 'a regular system', in which there was no vestige of a beginning nor prospect of an end, the balance between the attractive and repulsive forces, (namely, gravity and heat), had to be constant; moreover, in the absence of any contrary evidence, Hutton proposed that the total power, and not merely its balance, is always conserved. Only this could fully satisfy his belief that in Nature there is wisdom, system and consistency.

Hutton's own last exposition of his theory was in book form: *Theory of the earth* (1795). There, he discussed the place of power and the earth's living nature more fully and poetically than anywhere else. His aim was simply to investigate 'form, quality or active power'[23] in Nature; his achievement, which he called "this great, this interesting view", was to establish unequivocally the living, organismic nature of the earth.

> "But is this world to be considered thus merely as a machine, to last no longer than its parts retain their present position, their proper forms and qualities? Or may it be also considered as an organized body? Such as has the constitution in which the necessary decay of the machine is naturally repaired, in the exertion of those productive powers by which it had been formed.
>
> This is the view in which we are now to examine the globe; to see if there be, in the constitution of this world, a reproductive operation by which a ruined constitution may be again repaired, and a duration or stability thus procured to the machine.... If no such reproductive power, or reforming operation, after due inquiry, is to be found in the constitution of this world, we should have reason to conclude that the system of this earth has either been intentionally made imperfect, or has not been the work of infinite power and wisdom".[24]

To deny his view of the earth was therefore to deny the traditional attributes of God. To accept his view accorded well with theology. This was an important point for Hutton and for the future reception of his theory since, as various scholars have discussed recently,[25] there were strong theological objections that could legitimately, and were, raised against his theory, especially against his conclusion that "we find no vestige of a beginning, no prospect of an end".

Hutton also believed that his system permitted a wider methodology for science than mere induction, namely by using final causes.[26] And when he wrote about the efficient causes in his system, he did not inquire into their essential natures, but was interested solely in whether they were agents natural and necessary for the earth:

> "The answer to this is plain: These operations of the globe remain at present with undiminished activity, or in the fullness of their power".[27]

Although this conclusion about the persistence of the earth's powers was important, it was concerned only with the *effects* of power; as he wrote in his 1788 paper,

> "We do not now enquire into the nature of those powers, or investigate the laws of light and heat, of cold and condensation, by which the various purposes of this world are accomplished; we are only to mention those effects which are made sensible to the common understanding of mankind...."[28]

A full analysis of the nature of those powers came in his treatises on light and heat.

One of Hutton's most explicit discussions of the organismic nature of the earth concerned the four levels in Nature: first was the animal system, then the vegetable system, the system of the earth, and finally the mineral system. Each system or level could perpetuate itself by using any parts that it required of the other systems; each system was alive; Hutton therefore proposed to

> "... consider how men of science, in examining the mineral state of things and reasoning from those appearances by which we are to learn the physiology of this earth, have misled themselves with respect to physical causes".[29]

In the final chapter of his treatise, he left no doubt about the physiological anchorage of his world-view; in the circulation of the matter of the globe, in its beautiful oeconomy, in its parts being repaired as soon as they are wasted, it was exactly like the body of an animal. In saying that his theory employed ideas which were essentially physiological, and which had featured in his medical dissertation, I do not mean that there were no other sources for such ideas than his own medical background. One particularly probable source was Plato, for as Ellenberger[30] has pointed out, Hutton was well read in classical literature. He must have known those passages in the *Timaeus* where Plato wrote of the living world, a world ever active:

> "On this wise, using the language of probability, we may say that the world came into being - a living creature endowed with soul and intelligence by the providence of God.
>
> This being supposed, let us proceed to the next stage: In the likeness of what animal did the Creator make the world?"[31]

Also

> "... the body of the world was created, and it was harmonized by proportion, and therefore has the spirit of friendship; and having been reconciled to itself, it was indissoluble by the hand of any other than the framer".[32]

Another of Plato's requisites for the earth was "... that it should be free from old age and unaffected by disease".[33] And Plato's complete view was that "... The world has received animals, mortal and immortal, and is fulfilled with them, and has become a visible animal containing the visible - the sensible God who is the image of the intellectual, the greatest, best, fairest, most perfect - the one, only begotten heaven".[34]

In emphasizing Hutton's organismic view of the earth, I have done nothing original.[35] What I have attempted, which no-one else seems to have done, has been to show that his physiological pattern of thought was so persistent throughout his geological system, that it most probably arose from his medical education and especially from certain paradigms contained in his doctoral dissertation; and that his thoughts about the animal oeconomy either generated, or at least guaranteed, his system of the earth - a system which (as the next section will show) was essentially one of maximum economy of power, and where matter did not exist as hard, particulate substance.

In addition to his geological works, Hutton published three others: *Dissertations of different subjects in natural philosophy* (1792),[36] *A dissertation upon the philosophy of light, heat and fire* (1794),[37] and a very long philosophical treatise called *An investigation of the principles of knowledge, and of the progress of reason, from sense to science and philosophy* (1794).[38] In the two treatises on natural philosophy the main topic was heat, for he believed that the main challenge facing both science and philosophy was to investigate what happens whenever we experience light, heat and cold. For this he had two reasons. Firstly, much experimentation had been done recently on heat: Joseph Black's study of latent heat intrigued Hutton and they had become firm friends; later, Horace Bénédict de Saussure (1740-1799) and Marc-Auguste Pictet (1752-1825) experimented on heat and Pictet apparently showed that cold could be reflected, a startling claim since cold was commonly supposed to be only a low degree of heat and not an agent in its own right; Johann Heinrich Lambert (1728-1777) and Jean André de Luc (1727-1817) had asserted that heat is material, and in Hutton's opinion this had also been a gross error of the French anti-phlogiston chemists and was one reason why he remained a staunch phlogistonist, despite his recognizing the skillfullness of Lavoisier and his followers. Secondly, heat was indispensable for Hutton's geological theory and he therefore felt obliged to study its nature and origin. In this sense, his treatises in natural philosophy were essentially footnotes to his *Theory of the earth*.

Having examined all the major studies recently done on light and heat, Hutton concluded that the primary source and form of these two agents was sunlight; he sometimes used 'solar emanation' and 'solar substance' as synonymous terms for this force of sunlight. Light, heat, electricity and sunlight seemed to him to be 'commutable' agents, all different manifestations of solar substance.[39] Such agents were attributable to two fundamental types of power of force; in the 1794 *Dissertation* he wrote

> "In thus considering natural bodies as composed of principles which have power and energy, we may now observe that there are fixed and incommutable powers or principles in bodies, as well as those which are more or less changing and commutable. ... the final and incommutable principles of bodies are, first, that of gravitation and cohesion; and secondly, that of concretion.... The moveable and commutable principles, again, are the two different species of heat, that is, the heat of fluidity and the heat of expansion.... But those commutable principles of body are also mutable in relation to the general system of bodies, in the regulated motions of which we percieve design and wisdom".[40]

Nature thus ran upon two types of force - attraction and repulsion. The former, which Hutton often called 'gravitating matter', existed in three forms - the Newtonian force of gravitation, the force of cohesion which held the elemental components of a substance together, and the force of concretion. The repulsive force, whose generic name was 'solar substance', occurred as light, specific and latent heats, and the agents of electricity and fluidity.

Since Hutton believed heat to be a power, he was sharply critical of the French anti-phlogiston theory and gave an impressive defence of phlogiston; but by phlogiston he meant no material, albeit imponderable, body, but a power derived from the 'solar substance'.[41] There were at least four important, and sometimes radical, conclusions that he reached on light and heat and that were to be commented upon by other natural philosophers: 1) That the universe consists ultimately of only two principles - the powers of attraction and repulsion in their several forms. This theory resembled Boscovich's though Hutton rarely even mentioned him. 2) That some of men's common ideas in natural philosophy would have to be revised; one such was weight, others were inertia and momentum. For instance, since he denied the existence of hard, impenetrable matter, weight could not be a just measure of the absolute quantity of matter in any body; indeed,

> "... it may only indicate the quantity of gravitation which is not then opposed and balanced by the separating power; ... Here, then, would be a principle of levity, by which bodies might be affected, as well as a principle of gravity".[42]

In his analysis of inertia, Hutton used as his basic premise nothing short of the conservation of force. This was the only occasion when he stated that principle so explicitly and envisaged it so clearly as more than just the Cartesian idea of conservation of motion. Moreover, he attempted, albeit in very rudimentary form, a distinction between what we would call force and energy. So important is this passage as an illustration of Hutton's deep concern over dynamics, that it merits lengthy quotation:

> "Inertia is properly a quality of natural bodies, in like manner as is weight. It is not a moving, nor a resisting, power properly speaking, but a capacity for persevering in a state, whether of motion or of rest, but without having an active, power to resist a change. Without making this distinction between power and capacity, it is impossible to understand the nature of *inertia*.

Inertia is a law of action and passion, by which motion is translated from one body to another, as well as continued in the body which has been made to change its place. ... let us suppose a certain species of matter, which did not gravitate, to have inertia. What would be the nature of this thing? If, according to the common notion of matter, this be a thing which is extended, then it is a body with magnitude and figure, and now we may consider it in relation to gravitating bodies which have *inertia* and *momentum*.

Let the body which does not gravitate be impelled by the body moving with momentum; I ask, In what manner, or according to what rule, is the velocity of the moving body to be divided between the two bodies? According to the rule of our experience, the gravitating body should move the opposing body, without losing any of its own velocity; and this would be the creation of motion without a cause, or the action of one body upon another without any reaction, which is inconsistent with the actual course of nature.

Let us again suppose the gravitating body at rest, impelled by the body which moves with *inertia* but without *momentum*. In that case, the two bodies must either move together, or both rest. If the bodies move, here is motion created, and momentum, from nothing .... If again, the two bodies do not move, but remain at rest in conjunction, here is motion annihilated, or action ended without reaction or effect. Thus we are led into conclusions which cannot be admitted .... It has now been shown that this thing, moving only with inertia, could not move any of the bodies of the material universe, without creating *force* which it has not, as having no weight".[43]

That this argument applied to other processes as well as to velocity and momentum, was apparent from his adding that neither heat nor light act by their weight, figure or volume, and that they are not physical bodies, but powers.

3) In addition to his belief in the non-creation of power, Hutton believed that all powers of attraction are commutable or correlated by virtue of their common origin in 'gravitating matter', and that all repulsive powers are correlated by virtue of their common origin in 'solar substance'.[44] 4) He concluded that heat is not a direct product of fire, but that phlogiston or solar substance is produced first, and then heat. Much of the non-theological criticism of his geological theory, as various scholars have shown recently,[45] focussed on the nature of his heat. His idea that heat was a power, sometimes dormant, sometimes active, which could become manifest without the accompaniment of combustion struck some contemporaries as sheer fantasy; one critic[46] complained that his entire theory of the earth depended upon the supposition of a perpetual central heat, capable of melting limestone and elevating continents, yet generated and supported without combustion - a substance with which the critic, at least, had never had any acquaintance. This criticism was inaccurate, for Hutton had asserted that the earth's heat derived initially from sunlight, and that the commutations of solar substance, the earth's heat and animal and vegetable life were the principal functions of the macrocosmic oeconomy, whose grand final cause was life itself. The following extract from his 1794 *Dissertation* illustrates his vision admirably:

> "It is the light of the sun which is here stored up on the substance of vegetable bodies, as fixed light or phlogiston, the principle of fire. Consequently, however this light of our fire may appear to differ in some small respects from that which comes immediately from the sun, we cannot suppose them as any way essentially different, but must conclude that they are fundamentally the same, that the light of the sun ... is continually flowing into this planet, for the purpose of actuating the terraqueous system of this earth, and for enlivening animal and vegetable bodies".[47]

Despite the criticisms of Hutton's ideas on heat and his theory of the earth, his consuming interest in dynamical agents was firmly within the British tradition which had grown from Newton's work, which had been and was still discussed by British philosophers, (one of whom, Dugald Sterart, was in Edinburgh contemporaneously with Hutton), and which had been an important feature of 18th century British chemistry (to wit, the works of John Keill, Stephen Hales (1677-1761) and Joseph Black), and of British physiology. One scholar suggested recently that the interest in latent heat during the late 18th century among British natural philosophers was partly responsible for the success of Boscovicheanism.[48] Latent heat, which Hutton probably learned of directly from Joseph Black, became an integral part of his earth theory and therefore helped to avail him of the Boscovichean tradition for constructing his own theory of matter. Not only did his matter accord well with certain key ideas in his doctoral dissertation, but it may reasonably be suggested to have been at the foundation of his theory of the earth. Although his matter theory never became popular, it did impress a few natural philosophers, of whom John Playfair and Humphrey Davy are now to be discussed.

John Playfair (1748-1819)

Playfair moved in the same academic circles in Edinburgh as Hutton, and was professor of mathematics there when he published his *Illustrations of the Huttonian theory of the earth*[49] in 1802. Playfair greatly admired Hutton's earth theory. However, what historians have not pointed out was Playfair's admiration of, and indebtedness to, his theory of matter, which can be seen in Playfair's own *Outlines of natural philosophy*.[50] Despite the fact that many parts of that work read like a didactic textbook of physics (which is largely what was intended), the author did raise some polemic and philosophical points. Tucked away among the definitions and terse statements of physical principles, there were propositions which revealed Playfair's own matter theory, which resembled Hutton's and Boscovich's. For instance, in section 2, 'Properties of matter', there was that seminal sentence from Newton's *Opticks*:

> "It is probable that even in the densest bodies, the quantity of solid matter is very small, compared with the quantity of empty space".[51]

Playfair developed Newton's speculation in his section on dynamics, where he proposed that bodies could be treated quite satisfactorily as mere points of power.[52] But the nature of power or force was itself an enigma, and the best that Playfair could do was to give a kinematic definition, $F = \frac{\dot{v}}{\dot{t}}$, adding that

> "It has been disputed, whether this expression of the force be a necessary truth, or one known only from experience. [D'Alembert, Elémens de Phil. Melanges, tom. 4, p.197]. It seems, however, to be a definition, or an assumption, and not a theorem. We have no distinct idea attached to the word FORCE, which we can compare with that which is conveyed by the formula $\frac{\dot{v}}{\dot{t}}$, in order to see whether there is a necessary agreement or not. But as the quantity $\frac{\dot{v}}{\dot{t}}$ is of great importance, and of frequent occurence in mechanical investigations, it is convenient to have a term to denote it .... The word FORCE has in reality, in dynamics, no other signification than this; ... and an entire treatise of dynamics might be written, in which the word FORCE would not once occur".[53]

Playfair rounded off his treatise with a philosophical analysis of force, examining in particular the question that Newton had posed - whether gravitation with its inverse square law is an essential property of all matter. Since all attempts to reduce gravitation to something more fundamental had failed, Playfair believed it to be an essential and true agent in Nature. But that was not the end of the issue, for in the very last paragraph of the treatise he forecast what future investigations into power would yield. That forecast was soon to be fulfilled magnificently.

> "If, on the other hand, we consider how many different laws seem to regulate the other phenomena of the material world, as in the action of Impulse, Cohesion, Elasticity, Chemical Affinity, Crystallization, Heat, Light, Magnetism, Electricity and Galvanism, the existence of a principle more general than any of these, and connecting all of them with that of Gravitation, appears highly probable.
>
> The discovery of this great principle may be an honour reserved for a future age, and Science may again have to record names which are to stand on the same levels with those of NEWTON and LAPLACE. About such ultimate attainments, it were unwise to be sanguine, and unphilosophical to despair".[54]

If Hutton's theory of matter, or one very similar to it, influenced Playfair's own ideas on matter, why did he not cite Hutton? Why did he cite Boscovich once only in his *Outlines*, and then on a tame point which had nothing to do with his dynamics? And why was his most readily acknowledged mentor in dynamics, especially in the philosophical principles of that science, Leibniz? The first point to realize is that Playfair's treatise was a text in science, and not one in natural philosophy; it was full of succinct definition, mathematical formulae and careful, unspeculative discussion. Clearly, he believed that whatever merits lay in the theories of Hutton and Boscovich - and he must have been well acquainted with them - the problem of power and force would require mathematical investigation if a lasting solution was to be found.

That was surely why he cited Newton, Leibniz, Laplace, D'Alembert and Euler, but never Hutton, and Boscovich only once, and why, though he hinted at a Huttonian or Boscovichean-type theory of matter, it was not discussed seriously except at the very end. Even then, Hutton and Boscovich were not mentioned explicitly, for he predicted that the discoverer of the all-embracing principle of power would be another Newton or Laplace, namely, a philosopher who could handle the issue with the rigour of mathematics.

The next philosopher to be discussed did not approach the problem of power in the rigourously mathematical way that Playfair predicted, but he did contribute enormously to its experimental and theoretical analysis. That philosopher was Humphrey Davy.

Humphrey Davy (1778-1829)

Davy's introduction to science had been his reading of Lavoisier's *Traité Élémentaire de Chemie* in the original French at the age of nineteen, while apprentised to a surgeon-apothecary in Cornwall. Thereafter, Davy educated himself in natural philosophy and particularly in chemistry. From October 1798 until February 1801 he worked under Dr. Thomas Beddoes (1760-1808) at his experimental medical institute in Bristol, and Beddoes' approach to medicine, specifically his interest in the curative qualities of gases, inevitably influenced Davy. Davy's earliest published papers appeared in a book edited by Beddoes in 1799; they were 'Experimental essays on heat, light and the combinations of light, with a new theory of respiration, and observations on the chemistry of life',[55] and 'Experimental essays on the generation of phosoxygen (oxygen gas), and on the causes of the colours of organic beings'.[56] Those essays will be discussed first, to show how certain major issues of his later writings, particularly power, occurred already in his first publication, and also to show that he had studied Hutton's theory of matter by 1799 and had found it stimulating. Then, Davy's *Elements of chemical philosophy* (1812)[57] will be discussed, where his earlier views, particularly on the composition of matter, became much more explicit. Thirdly, his Bakerian lectures to the Royal Society of London will be discussed vis-à-vis the evolution of his ideas on power and matter from 1806 until 1826. His final stand on those themes will be shown in extracts from his last work, *Consolations in travel* (1830).[58]

Davy's essay on heat, light and the combinations of light was remarkable. Though only twenty years old, he showed considerable skill at discussing fundamental problems in natural philosophy, and despite his poetic, Romantic background and consequent willingness to give vent to his fertile imagination, he described many careful experiments that he and others had done. These experimental details must have given considerable authority to the hypotheses he advanced in the essays. One of these hypotheses was the purely dynamic nature of heat. Like Hutton, he considered heat a manifestation of light under certain conditions of motion, and he rejected Lavoiser's theory of combustion for much the same reasons as Hutton had: namely, that heat was not material and that the French chemists had neglected the rôle of light in combustion and respiration. Davy considered atmospheric oxygen, which he called 'phosoxygen', to be a combination of elemental oxygen plus light, and that the initial event in combustion was liberation of light; heat was a secondary event, dependent on the release of light.[59] Hutton, whom Davy cited[60] for his combustion theory, had espoused a slightly different explanation, namely, that light occurred in combination with combustible matter and was released in various forms from it - and not from the oxygen. They both emphasized the central rôle of light, not only in combustion but in other natural phenomena too. Indeed, for Davy, light was a sort of protean spirit which related all Nature's agents and phenomena one to another:

> "Bodies perfectly black must subtract so much of the repulsive motion of light, as to deprive it of its repulsive projectile form. The electric fluid is probably light in a condensed state, that is, not supplied with the repulsive motion sufficient to give it repulsive projection. Its chemical action upon bodies is similar to that of light, and when supplied with repulsive motion by friction, or by contact of bodies from which it is capable of subtracting it, it takes the repulsive projectile form, and becomes perceptible as light. It is extremely probable that the great quantity of this fluid almost everywhere diffused on our earth, is produced by the condensation of light, from the subtraction of its repulsive motion at the poles, by the revolution of the earth on its axis, and given off in the form of repulsive, projectile

> light, whilst a quantity equal to that given off from its equilibrating principle is supplied continually from the other parts of the globe. No more sublime idea can be formed of the motions of matter, than to conceive that the different species are continually changing into each other. The gravitative, the mechanical, and the repulsive motions appear to be continually mutually producing each other, and from these changes all the phenomena of the mutation of matter probably arise".[61]

As had Hutton, Davy saw light as the *primum movens* in the oeconomy of Nature, as that agent which united the organic and inorganic realms, and as that field in which the most important discoveries would be made.[62] Davy's vision of the universal power of light was even broader than Hutton's; it embraced Man as well as Nature, and led him to God, whose Will was the ultimate law and source of power.

> "Light enters into the composition of living bodies. To understand these combinations is of infinite importance to man. On the existence of this principle in organic compounds, perception, thought and happiness appear to depend.
>
> Life, then, may be considered as a perpetual series of peculiar corpuscular changes; and the living body as the being in which these changes take place. Perceptions, ideas, pleasures and pains are the effects of these changes. They are consequently found to be continually varying. The laws of mind, then, probably are not different from the laws of corpuscular motion. Every change in our sensations must be accompanied by some correspondent change in the organic matter of the body. These changes, an extensive and philosophic chemistry may enable us to estimate.

> Thus, essential then is LIGHT to perceptive existence. All organic sensitive beings with which we are acquainted appear totally unable to exist without phosoxygen.
>
> We may consider the sun and the fixed stars ... as immense reservoirs of light destined by the great ORGANISER to diffuse over the universe organization and animation. And thus will the laws of gravitation, as well as the chemical laws, be considered as subservient to one grand end, PERCEPTION. Reasoning thus, it will not appear impossible that one law alone may govern and act upon matter: an energy of mutation, impressed by the will of the Deity, a law which might be called the law of animation, tending to produce the greatest possible sum of perception, the greatest possible sum of happiness".[63]

These ideas on the unity of Nature, the commutability of the powers of the universe, the vast scope for chemistry and its potential benefit to mankind, remained with Davy throughout his life and were discussed in even his most rigourously experimental writings. The sources for these ideas would take a whole book to discuss.[64] May it suffice here to mention three sources: Firstly, Davy had access to various libraries during his years in Cornwall before working under Beddoes; according to one of his recent biographers,[65] he read Locke, Hume, Hartley, Berkeley and other Scottish philosophers, including Hutton, and apparently delighted in metaphysics. Secondly, at Bristol he became a close friend of Coleridge and Robert Southey (1774-1843), and from their circle he acquired a *Naturphilosophie* vision of Nature.[66] Like them, he had a poet's view of things, which was for him no less valid and no less verifiable than the careful constructs of the scientist. Thirdly, Thomas Beddoes' inspiring ideas on the rôle of natural philosophy, which he elaborated in his introductory essay to the 1799 treatise, greatly impressed Davy. For instance, Beddoes asserted that

> "The science of human nature is altogether incapable of division into independent branches. ...the moralist and the metaphysician will each to a certain extent encroach upon the province of the physiologist. Every code of morals must ground its precepts on a comprehensive view of the laws that regulate feeling, and deliver the conditions of an offensive and defensive league, having for its object the well-being of individuals. Without accurate ideas, therefore, of the causes that affect the personal condition of mankind, how is it possible to conceive any progress in genuine morality? And will not every addition to this brance of knowledge necessarily tend to purify morals - that is, to introduce into the social compact covenants more beneficial to the parties? Without reference to the body, it is equally impossible to unfold the nature of the mind. Physiology therefore - or more strictly, biology - by which I mean the doctrine of the living system in all its states, appears to be the foundation of ethics and pneumatology".[67]

The aim of Beddoes' book was to promote man's knowledge of himself, in the belief that education was an apprenticeship to happiness.[68] Davy shared that belief.

One ambiguity in Davy's essay was the constitution of matter. Powers were important, but he also employed material corpuscles. Unlike Hutton, who used the word 'substance' much as we today use the word 'something', namely to denote any existent whose nature need not be specified, Davy clearly meant something substantial whenever he used 'substance', 'corpuscle' or 'body'. For that reason, he was emphatic when he believed something to be immaterial, such as heat.[69] Light he considered corpuscular, often calling it 'repulsive projectile motion'; in this he differed from Hutton, citing Erasmus Darwin's *Zoonomia*, Newton's *Opticks* and some experiments of his own for support.[70] However, it is worth repeating that he cited Hutton whenever he wished to refute Lavoisier's combustion theory, Lavoisier's error having been "... the assumption of the imaginary fluid caloric, and the total neglect of light".[71]

Davy's second essay in Beddoes' collection was totally different from the first, being concerned largely with experimental chemistry and scarcely with the problems of light, heat etc. Together, these essays show that he was a corpuscularian, though acquainted with the arguments of at least one recent dynamical philosopher, Hutton. One Davy scholar, D.M. Knight,[72] has shown that he was still a corpuscularian during his early years at the Royal Institution, although he was then becoming acquainted with Boscovicheanism: for instance, his predecessor in the chair of chemistry, Thomas Garnett (1766-1802) was discussing Boscovich in his lectures in 1801:

> "Our ideas of impenetrability less certain than we we have suspected. It is highly probable that the tangible particles of matter are not in contact, but are connected by mechanical forces, which, like gravity, act at a distance. Theory of Father Boscovich".[73]

In the same year, the *Philosophical Magazine* published a brief biographical article on Boscovich, and Priestley had earlier discussed his work. According to Knight, by his first Continental tour in 1815 with Lady Davy and his assistant , Michael Faraday (1791-1867), Davy was a convinced Boscovichean. Here, we shall see certain stages in the development of his theory of matter.

In his *Elements of Chemical Philosophy* (1812), Davy used his previous key ideas on the constitution of matter, though modified by recent and important experiments. The main cause of these modifications was the new experimental study of the voltaic apparatus. Davy was one of the earliest investigators of voltaic electricity, and by 1806, when he gave his first Bakerian lecture, he had become the great authority on it. His studies showed that a strong connection, indeed a rigorous quantitative one, existed between voltaic action or power and its various effects of heat, light and chemical change. As he wrote in his historical introduction to the *Elements*,

> "Bodies combine with a force, which in many ways is correspondent to their power of exhibiting electrical polarity by contact; and heat, or heat and light, are produced in proportion to the energy of their combination. Vivid inflammation occurs in a number of cases in which gaseous matter is

> not fixed; and this phenomenon happens in various instances without the interference of free or combined oxygen".[74]

As in 1799, he was eager to emphasize the importance of power in his dualistic view of matter; for instance, the greatest praise went to Newton for "his distinct philosophical elucidation of the powers which produce the changes and apparent transmutations of the substances belonging to the earth".[75] Later in the book he returned to this transmutation theme, calling it a "sublime chemical speculation, sanctioned by the authority of Hooke, Newton and Boscovich".[76] This was his only mention of Boscovich, but Boscovichean ideas occurred elsewhere, as in the introduction where he asserted that whether matter is corpuscular, or merely physical points endowed with attraction and repulsion, still the same conclusions would be reached about the powers and quantities whereby reactions occur.

Davy suspected strongly by now that Nature's powers were correlated with one another, for his electrochemical researches indicated this. One important consequence was that he now suspected light was a wholly dynamical phenomenon; he cited an experiment by Rumford, who

> "... has lately shewn that the quantity of light emitted by a given portion of inflammable matter in combustion is proportional in some high ratio to the elevation of temperature; and that a lamp having many wicks near each other, so to communicate heat, burns with much more brilliancy in proportion to the consumption of oil than the Argand's lamps in common use".[77]

Davy was still unwilling to commit himself to a theory of light; he quoted a long passage from Newton's *Opticks* showing how well a corpuscular theory accounted for phenomena like double refraction. Nonetheless, his new readiness to discuss light as a dynamical agent was a significant departure from 1799 and brought him more in line with Hutton. Moreover, his opposition to the French anti-phlogistonists had become even stronger, for recent researches seemed to show that no peculiar substance, or form of matter, was necessary for light, that it was a general result of the reactions of any substances possessing strong chemical or electrical properties, and that it was always consequent on intense motion. There was a clear kinship between this view and Hutton's combustion theory, (although Hutton was not cited explicitly in the *Elements*): Hutton had also asserted that the source of light and heat was within the combustible matter itself, and not in the oxygen; that this source, which he called 'solar substance', was essentially the chemical power which held combustible matter together; and that light and heat were powers of common origin, though not themselves identical.

If Davy's ideas were so similar to Hutton's, why did he not admit his indebtedness? One reason might have been that Hutton was not an experimentalist, whereas Davy valued experimentation and used it to check his Romantic exuberance. This was what set him apart from his friends, Shelley, Coleridge and Wordsworth, and whilst Wordsworth wrote that

"Poetry is the breath and finer spirit of all knowledge; it is the impassioned expression which is the countenance of all science",[78]

Davy proposed that

"A few undecompounded bodies, which may perhaps ultimately be resolved into still fewer elements, or which may be different forms of the same material, constitute the whole of our tangible universe of things. By experiment they are discovered, ... and experiment is, as it were, the chain that binds down the Proteus of Nature, and obliges it to confess its real form and divine origin".[79]

Davy was willing to entertain ingenious hypotheses and metaphysical speculations, but these had to be corroborated by experimental evidence before being admitted to his serious chemical writings. Where he was not writing as a chemist, but as a philosopher, he did commit such ideas to print more readily, as in his posthumous *Consolation in travel; or the last days of a philosopher* (1830). There, in dialogue VII, Eubathes said

"On the hypothesis of Boscovitch, which is well explained in the *Institutio Physica* of Mako, matter, as well as I recollect, is supposed to be composed of indivisible points endowed with attraction and repulsion, which are assumed to be both physical and chemical elements".

To which the Unknown, who stood for Philo-chemicus, replied:

"You mistake me if you suppose I have adopted a system like the HOMOOIA of Anaxagoras, and

> that I suppose the elements to be physical molecules endowed with the properties of the bodies we believe to be indecomposable. On the contrary, ... I consider them, with Boscovitch, merely as points possessing weight and attractive and repulsive powers; and composing, according to the circumstances of their arrangements either spherules or regular solids, and capable of assuming either one form or the other".[80]

Turning back to Davy's Bakerian lectures to the Royal Society of London, of which he gave six between the years 1806 and 1826 and in which he described the latest state of electrochemistry, we find that his principal persistent problem was to explain the source of voltaic power. Volta's own explanation of how his 'pile' and 'crown of cups' worked was that electricity was generated and maintained by the mere contact of dissimilar metals; since no activity could be discerned in a state of contact, Volta's hypothesis implied that the electrical effect might be *sui generis* and its continuance might be an instance of perpetual motion. Volta wisely did not elaborate much on this implication, but he did express excitement at it.[81]

Repeatedly, Davy was to refute this implication of Volta's hypothesis, and in so doing he inquired into the primary source of all phenomena associated with electricity - of electricity itself, of electrochemical activity, light and heat. His thoughts are to be found especially in those sections of the Bakerian lectures where he discussed the theoretical infrastructure of his work. Repeatedly, he suggested that a single power subsumed the different phenomena and powers exhibited in electrochemistry; he also warned that that hypothesis was unproven and would probably long remain so. So, in the first lecture he said:

> "In the present state of our knowledge, it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies, after being brought into contact, should be found differently electrified; its relation to chemical affinity is, however, sufficiently evident. May it not be identical with it, and an essential property of matter?"[82]

In the third lecture (1809), he ended on the same theme:

> "I venture to hint at these notions; but I do not attach much importance to them; the age of chemistry is not yet sufficiently mature for such discussions; the more subtile powers of matter are but just beginning to be considered; and all general views concerning them must as yet rest upon feeble and imperfect foundations".[83]

His most sanguine discussion of this theme came in the last lecture (1826). By then, he had the satisfaction of knowing that the electrochemical hypothesis he had proposed twenty years earlier was still viable, and in view of the recent discoveries of Hans Christian Oersted (1777-1851) and Domenico Morichini (1773-1836) he believed that electrochemistry could be connected with a whole new range of phenomena, and that

"... many of the complicated phenomena of corpuscular changes, now obscure, will ultimately be found to depend upon the same causes, and to be governed by the same laws; and that the simplicity of our scientific arrangements will increase with every advance in the true knowledge of nature".[84]

Even in that sixth lecture, Davy felt it necessary to refute again Volta's original contact hypothesis, which was so opposed to his own belief in the inter-dependence of Nature's powers.[85]

Another feature of Davy's Bakerian lectures, and one which is a major reason for including him in this dissertation, was that whenever he discussed the future applications of electrochemistry, the foremost were in animal and plant physiology. This is hardly surprising, since his first ambition was to study medicine, and in 1804 he was enrolled as a commoner at Jesus College, Cambridge, for that purpose. The medical and physiological potential of his work was often in his mind, as his first lecture shows:

"As acid and alkaline substances are capable of being separated from their combinations in living systems by electrical powers, there is every reason to believe that by converse methods they may be likewise introduced into the animal economy, or made to pass through the animal organs: and the same thing may be supposed of metallic oxides; and these ideas ought to lead to some new investigations in medicine and physiology".[86]

Davy himself did electrochemical experiments on plant and animal tissue, particularly on the Mediterranean torpedo fish, and his prediction of the impact of electochemistry on medicine and physiology was to be fulfilled amply.

He concluded his first lecture with an inspiring vision of the universality of electricity, a vision in keeping with his Romantic background and which revealed his keen awareness of the sheer power he was putting into man's hands: he could easily imagine a limit to the electrical energies that natural substances possessed, whereas the powers of man's electrical instruments seemed capable of indefinite increase; Nature would soon be at man's mercy.[87] Indeed, Davy was a magnificent vindication of Bacon's belief that in acquiring knowledge, man would acquire power. Davy was exhilarated by it; man would become Prometheus, and thus would his divine parentage (Adam was created in the image of God) be fulfilled. He put these aspirations most eloquently in dialogue V of the *Consolations in travel*; the dialogue was called 'The Chemical Philosopher', and in the words of the Unknown he said

> "How different is man in his highest state of cultivation! every part of his body covered with the products of different chemical and mechanical arts ... extracting metals from the rude ore and giving to them a hundred different shapes for a thousand different purposes; ... making the winds carry him on every part of the immense ocean; and compelling the elements of air, water and even fire, as it were, to labour for him;

concentrating in small space materials which act as the thunderbolt and directing their energies so as to destroy at immense distances; blasting the rock, removing the mountain, carrying water from the valley to the hill; perpetuating thought in imperishable words, rendering immortal the exertion of genius and presenting them as common property to all awakening minds - becoming, as it were, the true image of divine intelligence receiving and bestowing the breath of life in the influence of civilization".[88]

"In short, in every branch of the common and fine arts, in every department of human industry, the influence of this science /chemistry/ is felt, and we may find in the fable of Prometheus taking the flame from heaven to animate his man of clay, an emblem of the effects of fire in its application to chemical purposes in creating the activity, and almost the life, of civil society".[89]

That power was in the offing, above all else, can be seen in this final extract:

"And the inventions connected with the steam engine, at the same time that they have greatly diminished labour of body, have tended to increase power of mind and intellectual resources. Adam Smith well observes that manufacturers are always more ingenious than husbandmen; and manufacturers who use machinery will probably always be found more ingenious than handicraft manufacturers".[90]

The dialogue of 'The Chemical Philosopher' was nothing short of a paean of praise to power, power in both a purely scientific and a sociological setting. From the powers of Nature, Davy saw man with his chemical skills and his knowledge of the correlations among those powers, making and wielding his own personal power. Man did not create his own power, for it existed already as Nature's powers; in this respect, man would always be below the gods, and like Prometheus he could borrow their powers for his own ends. Such was the glittering prize that Humphrey Davy, the dying philosopher, had won and was bequeathing to mankind.

Various scholars have examined the way in which Michael Faraday took up certain of Davy's ideas. D.M. Knight[91] and Peirce Williams,[92] in particular, have examined Faraday's conversion to Boscovicheanism; and although it is tempting to discuss their work here, we must desist, for we would be straying too far from the purpose of this thesis. Besides, the physiologists who will be discussed in the following chapters were far better acquainted with Davy's speculations on the nature of matter than with Faraday's. Faraday's first public indication of his Boscovicheanism was in 1844, which is close to the end of the period covered by this thesis.

NOTES TO CHAPTER 4.

1. C.C. Gillispie, *Genesis and geology, the impact of scientific discoveries upon religious beliefs in the decades before Darwin*, New York, Harper & Row, 1959, p.74.

2. I have not yet been able to study Hutton's original Latin dissertation. For my understanding of that work, I am chiefly indebted to Professor François Ellenberger for discussions I had with him and for his paper 'La thèse de doctorat de James Hutton et la rénovation perpétuelle du monde', *Annls.Guébhard-Séverine*, 1973, 49:497-533.

3. J. Playfair, 'Biographical account of the late James Hutton,F.R.S., Edinburgh', *Trans.R.Soc.Edinb.*, 1805, 5(part 3):39-99. See p.46.

4. *Op.cit.*, Note 2 above.

5. J. Hutton, *Theory of the earth*, Edinburgh, Creech, 1795, pp.545-546.

6. J. Hutton, *Dissertatio physico-medica inauguralis de sanguine et circulatione microcosmi*, Lugduni Batavorum, 1749, para. II-III.

7. *Ibid.*, para. XL-XLVIII.

8. *Ibid.*, para. XLIX-LXVII.

9. *Ibid.*, para. LXVIII-LXXII.

10. *Ibid.*, para. LXXVIII-LXXIX.

11. *Op.cit.*, Note 2 above, pp.514-515.

12. *Ibid.*, pp.510, 511 & 515.

13. Hutton, *op.cit.*, Note 6 above, para. II.

14. This theme is discussed at great length in Ellenberger's paper.

15. D. Stewart, in Sir W. Hamilton (editor), *The collected works of Dugald Stewart*, Edinburgh, Thomas Constable & Co., 1855, 5:96-100.

16. J. Hutton, *Abstract of a dissertation read in the Royal Society of Edinburgh, upon the seventh of March, and fourth of April, M,DCC,LXXXV, concerning the system of the earth, its duration and stability*, in facsimile reproduction in G.W. White, *Contributions to the history of geology*, V, Hafner Publishing Co., Darien, Connecticut, 1970. Since this abstract is reproduced in facsimile, I have given Hutton's original pagination in the following references.

17. *Ibid.*, p.28.

18. *Ibid.*, p.29.

19. J. Hutton, 'Theory of the earth; or an investigation of the laws observable in the composition, dissolution and restoration of land upon the globe', *Trans.R.Soc. Edinb.*, 1788, 1(Part 2):209-304.

20. *Ibid.*, p.212.

21. *Ibid.*, p.213.

22. *Ibid.*, p.273.

23. *Op.cit.*, Note 5 above, p.4.

24. *Ibid.*, pp.16-17.

25. D.R. Dean, 'James Hutton and his public, 1785-1802', *Ann.Sci.*, 1973, 30:89-105.
D.R. Dean, 'James Hutton on religion and geology, the unpublished preface to his *Theory of the earth* (1788)', *Ann.Sci.*1975, 32:187-193.
Also, P.A. Gerstner, 'James Hutton's theory of the earth and his theory of matter', *Isis*, 1968, 59:26-31.

26. *Op.cit.*, Note 5 above, p.40.

27. *Ibid.*, p.141.

28. *Op.cit.*, Note 19 above, p.213.

29. *Op.cit.*, Note 5 above, p.286.

30. *Op.cit.*, Note 2 above.

31. Plato, *Timaeus*, 30, c.

32. *Ibid.*, 32, c.

33. *Ibid.*, 33, a.

34. *Ibid.*, 92, c.

35. For instance, Dean and Ellenberger have discussed this theme; indeed any student of Hutton's philosophy cannot help but notice his organismic view. However, E.B. Bailey did not give much attention to this, preferring to concentrate on Hutton's pure geology. See E.B. Bailey, *James Hutton, the founder of modern geology*, London, Elsevier Publishing Co., 1967.

36. J. Hutton, *Dissertations on different subjects in natural philosophy*, Edinburgh, printed for A. Strahan and T. Cadell, London, 1792.

37. J. Hutton, *A dissertation upon the philosophy of light, heat and fire*, Edinburgh, Cadell, Junior and Davies, 1794.

38. J. Hutton, *An investigation of the principles of knowledge, and of the progress of reason, from sense to science and philosophy*, 3 vol., Edinburgh, printed for A. Strahan and T. Cadell, London, 1794.
One gap in my knowledge of Hutton is that I have not read this work. My time was limited, and the likelihood of adequately understanding this large philosophical treatise without spending a great amount of time on it seemed very slim to me. Eventually, I intend to study it. So far as I am aware from secondary literature on Hutton, the ideas in this philosophical treatise do not vitiate my interpretation of his work in natural philosophy.

39. *Op.cit.*, Note 37 above, pp.xvii-xviii and elsewhere.

40. *Ibid.*, pp.142-143.

41. *Ibid.*, p.249.

42. *Ibid.*, p.238.

43. *Ibid.*, pp.259-265.

44. For a succinct analysis of this aspect of Hutton's theory of matter, see P.A. Gerstner, 'James Hutton's theory of the earth and his theory of matter', *Isis*, 1968, 59:26-31.

45. *Ibid.*, and D.R. Dean, *op.cit.*, Note 25 above.

46. Anon, 'Review of Playfair's *Illustrations of the Huttonian theory of the earth*', in *Edinburgh Review*, 1802, 1:202.

47. *Op.cit.*, Note 37 above, pp.323-324.

48. L.P. Williams, 'Boscovich and the British chemists', in L.W. Whyte (editor), *Roger Joseph Boscovich*, London, George Unwin, 1961, p.159.

49. J. Playfair, *Illustrations of the Huttonian theory of the earth*, Edinburgh, Creech, 1802.

50. J. Playfair, *Outlines of natural philosophy*, Edinburgh, A. Neill & Co., vol.1, 1812, vol.2, 1814.

51. I. Newton, *Opticks*, 4th edition, London, William Innys, 1730, Bk.2, part III, proposition VIII.

52. *Op.cit.*, Note 50 above, p.15.

53. *Ibid.*, pp.40-42.

54. *Ibid.*, pp.340-341.

55. H. Davy, 'Experimental essays on heat, light and the combinations of light, with a new theory of respiration, and observations on the chemistry of life', in T. Beddoes (editor), *Contributions to physical and medical knowledge, principally from the West of England*, Bristol, Biggs & Cottle, 1799, pp.5-150.

56. *Ibid.*, pp.151-210.

57. H. Davy, *Elements of chemical philosophy*, London, J. Johnson & Co., 1812.

58. H. Davy, *Consolations in travel; or, the last days of a philosopher*, 1830, in John Davy (editor), *The collected works of Sir Humphrey Davy*, London, Smith, Elder & Co., 1839-1840, vol.9, pp.207-388.

59. *Op.cit.*, Note 55 above, pp.50-51.

60. *Ibid.*, p.64.

61. *Ibid.*, pp.46-47.

62. *Ibid.*, pp.51-52.

63. *Ibid.*, pp.144-147.

64. An excellent analysis of Davy's work is in Sir H. Hartley, *Humphrey Davy*, Wakefield, England, EP Publishing Ltd., 1972.

65. *Ibid.*, p.12.

66. *Ibid.*, pp.22-25.

67. T. Beddoes, *op.cit.*, Note 55 above, p.4.

68. *Ibid.*, p.5.

69. *Op.cit.*, Note 55 above, pp.50-51.

70. *Ibid.*, pp.46-47.

71. *Ibid.*, p.68.

72. D.M. Knight, *Atoms and elements*, London, Hutchinson, 1967, p.39.

73. T. Garnett, *Outlines of a course of lectures on natural and experimental philosophy*, London, Cadell & Davis, 1801, p.5.

74. *Op.cit.*, Note 57 above, p.39.

75. *Ibid.*, p.20.

76. *Ibid.*, p.364.

77. *Ibid.*, p.165.

78. W. Wordsworth in the preface to his *Lyrical Ballads* (1802).

79. *Op.cit.*, Note 57 above, p.375.

80. *Op.cit.*, Note 58 above, pp.387-388.

81. A. Volta, 'On the electricity excited by the mere contact of conducting substances of different kinds', *Phil.Trans.*, 1800, pp.403-431.

82. H. Davy, in J. Davy (editor), *The collected works of Sir Humphrey Davy*, London, Smith, Elder & Co., 1839-1840, vol.5, p.40.

83. *Ibid.*, p.138.

84. *Ibid.*, vol.6, p.306.

85. *Ibid.*, pp.330-331.

86. *Ibid.*, vol.5, p.53.

87. *Ibid.*, p.54.

88. *Op.cit.*, Note 58 above, p.352.

89. *Ibid.*, pp.353-354.

90. *Ibid.*, p.357.

91. *Op.cit.*, Note 4 above, pp.43-53.

92. L. Peirce Williams, *Michael Faraday*, London, Chapman & Hall, 1965.

Chapter 5 On power or force in the physiologies of six British physiologists of the early 19th Century: John Bostock (1773-1846), Charles Bell (1774-1842), Thomas Bateman (1778-1821), Thomas Southwood Smith (1788-1861), Marshall Hall (1790-1857) and William Pulteney Alison (1790-1859).

This chapter discusses the dynamical physiologies of several figures who were well-known and widely-read in their day. Their ideas on power were not identical, though one idea they held in common was that power or force, (which were usually still regarded synonymously), was the central concept in the animal oeconomy, and therefore that to comprehend that concept, especially vis-à-vis its operations in the inorganic world, would unfold the basis of life. Almost without exception, these physiologists eschewed non-dynamical explanations of life; they had little time for chemical explanations of vital functions, though they did appreciate the skillfullness of the chemical researches of Antoine Lavoisier (1743-1794), William Prout (1785-1850), Friedrich Tiedemann (1781-1861) and Leopold Gmelin (1788-1853); to them, chemistry did not seem a penetrating enough tool in physiology. One oft-cited example they gave of the limitations of chemistry was the study of 'animal heat'; Dulong and Despretz had shown that about 80% of an animal's warmth could be generated by the chemical process of respiration, but that left 20% unaccountable except by essentially dynamic agents like vital or nervous forces.

These physiologists were conspicuous for their reliance on experimentation done by their contemporaries and immediate precursors, and occasionally by themselves, for their avoidance of speculation and metaphysics, and for their reticence before advocating their own hypotheses. Even those of them who were not experimentalists took note of other men's experiments and were much less ready to theorize than men like Erasmus Darwin, Cullen and Brown had been. Physiology had become sober, if not quite a science, by the 1820s. However, they still used philosophical principles to bolster their physiological ideas, and Locke, Reid and Stewart were cited often to support their scientific arguments. The methods of the natural philosopher were not obsolete.

Their use of power will be discussed in three contexts, namely, in theories of fever, muscle action and blood circulation; these were key themes in their physiologies, and powers or forces seemed to be especially useful in elucidating them.

John Bostock, a Liverpool physician, was the first of the group to write a comprehensive treatise on physiology. As early as 1804 he began to gain a reputation with his *Essay on respiration*.[1] This was a careful review of recent British and Continental researches, in which Bostock endorsed the Lavoisierean doctrine. Despite the work that had been done on respiration, Bostock felt that physiology as a whole had scarcely progressed, and the purpose of his three-volume *Elementary system of physiology*[2] (1824, 1826 and 1827) was to promote "a systematic and connected view of modern physiology"[3], a deficiency which surprised him in view of the distinguished part which he believed British physiologists had played in unravelling the animal oeconomy. As one historian has mentioned recently[4], Bostock pitted himself vigourously against vitalism; consequently, he was interested in the relations between vital functions and external, physico-chemical powers. The question, whether vital activity could be explained by either purely inorganic agents or by some basic form of power common to both living and inorganic systems, directed the whole treatise. In the light of current knowledge he admitted, regretfully, that the correlation he had been seeking could not be proven. However, he did assert that vital activity was critically dependent on inorganic powers; in discussing muscular contractility, he pointed out that

> "... this power is never exercised without the agency of some direct, independent cause, to which the name of a stimulant has been applied; and we arrive at this conclusion, not merely from the general principle that every effect in nature must have its appropriate cause, ... but from actually observing that when we see muscular contraction taking place, and have an opportunity of examining all the previous circumstances, we can assign the exact event that has produced the contraction"[5].

To establish the need for an external stimulus for muscle activity was not, of course, to say that the size of the muscular action depended rigourously on the size of the stimulating power. To that end, he reviewed the caloric, chemical, electrical and other hypotheses of contractility, concluding that there was strong evidence for

> "... a very intimate connexion between the chemical composition of the fibre and its contractile power. But they do not prove anything besides this; they demonstrate that a connexion exists between the two circumstances, not that one is the cause of the other".[6]

Hence,

> "... in the present state of our knowledge, contractility ought to be regarded as the unknown cause of known effects, a quality attached to a particular species of matter possessed of properties peculiar to itself, and which we are not able to refer to any general principle".[7]

Although this conclusion denied that contractility was correlatable with chemical forces, it also denied its correlation with any general power of vitality. For Bostock, the source of contractility was an important question, still beyond the competence of physiology. However, he did imply that it is not a power *sui generis*, but depends somehow on the chemistry of muscle fibre and always needs a stimulus to effect it. However, unlike Cullen and Brown, he did not seem to enquire into the nature of that dependence. It could be said that in his assertion above, that every effect must have its appropriate cause, he envisaged a truly quantitative relation, whereby the size of the effect would equal the size of the cause.

In short, what did he mean by 'appropriate'? Cullen and Brown had been groping towards a semi-quantitative principle of dependence, but Bostock is more doubtful. But we can say that he did not write as if he thought the animal oeconomy was a *perpetuum mobile*; he was much interested in the interdependence of the powers and activities of the organism and toyed with the idea of all those powers depending on a single, fundamental, not necessarily vital, power; for instance,

> "The circulation has been stated to be the prime cause of all the rest, for it is that which carries to every part of the body the fluid which endows it with its vital properties and its appropriate powers".[8]

The possibility of an overall correlation of the vital powers with one another and with inorganic ones became most evident in his discussion of the mind-body relation, in which he asked if mental phenomena could be put in the same class as material ones. His conclusion was cautious and revealed his aspirations for physiology and the scope of the doctrine of power.

> "Upon the same grounds, therefore, that we conceive ourselves justified in supposing gravitation to be a property different from chemical affinity, I should maintain that mental are essentially different from physical phenomena .... But in the same way that we have discovered galvanism and magnetism to be modes of electricity, so future discoveries may assimilate mind to matter .... I will not

> presume to prescribe limits to our discoveries, either in physical or metaphysical philosophy, but it may be fairly argued that until such discoveries are made, ... the cause of truth and knowledge is more effectually served by arranging phenomena according to their actually ascertained differences, than by attempting to generalize possible, or even imaginary, resemblances".[9]

It is surely reasonable to suggest that, had Bostock been alive when the conservation of energy was enunciated formally in the late 1840s and when his professional acquaintances, Richard Fowler and William Carpenter, drew attention to its application to the vital economy, he would not have been surprised; for those ideas were emerging in his own physiology. Moreover, Bostock had been a close friend of Peter Roget during their student years in Edinburgh and they remained on close terms when practising; Roget was asserting the correlation and conservation of forces, especially with regard to the vital economy, during the 1830s. Bostock's *Elementary system of physiology* was one of the most popular textbooks in Britain until Baly's translation of Müller's *Physiology* (1840); he was therefore a central figure in British avitalistic, dynamical physiology, but he was sufficiently critical and 'scientific' to realize that the time was not ripe to declare a fully reductionistic theory of organic powers. He died a few months before Helmholtz read his paper 'Ueber die Erhaltung der Kraft'.

Charles Bell is an example of a medical theorist who acquired considerable reputation for experimental skill, and whose ideas therefore on the animal economy carried considerable weight among the critical physiologists of his day. Unlike the other theorists in this chapter, he rejected all hypotheses on connexions between organic and inorganic power, believing that the organism is truly a creator and destroyer of power. However, he deserves discussion in this thesis to show what arguments were pitted against the trend in physiological dynamics that I am trying to trace; to show how power was an important issue for him and constituted the basic distinction between living and inanimate matter; and because one of the contexts in which he discussed power, namely the circulation of the blood, was discussed by one of his eminent colleagues, Marshall Hall, and it will be instructive to compare their views.

Bell's two treatises to be discussed have deliberately not been chosen from his neurological work, for that has been studied already by medical historians;[10] a more original contribution would be to discuss two less studied treatises: his *An essay on the forces which circulate the blood* (1819)[11] and his beautifully written *Animal mechanics, or proofs of design in the animal frame* (1828).[12]

In the *Essay*, Bell declared himself a vitalist and went to great lengths to show that in the vital oeconomy God suspends the usual laws of inorganic mechanism. The most persuasive instance was the blood circulation, for if one considered the extent to which the body's tissues and the walls of the blood vessels exerted their attractive forces of chemical affinity and cohesion upon the blood, Bell thought that the heart would have to exert a greater force than it does, to keep the blood in motion. Therefore,

> "... are we to suppose that this universal attraction of the fluids and solids is negatived in the vessels of the living body, that the great Architect, instead of accumulating forces to overcome the vast resistance, has annihilated it, and rendered a smaller force sufficient to the end ...?"[13]

His reply was 'Yes'; that the physico-chemical powers within the organism are ruled by its needs; that the living body can originate its own notions, which ought "to lead us to consider it in a very different light from the class of inanimate objects around us";[14] and that in the circulation of the blood, man surveys a new world, where velocity and impetus have no sufficient cause and to which the laws of familiar things do not extend.[15]

It is worth emphasizing that, in denying sufficient causes for living motions, Bell implied that this contrasted with the dynamics of the inorganic world, where all effects should have sufficient causes. This contrast was discussed in his *Animal mechanics*, a work of natural theology as well as anatomy and physiology. That treatise shows us that Sir Charles Bell, (as he was by then), not only understood his subject as an experimentalist, but had contemplated it deeply and saw himself continuing the tradition of John Ray (1627-1705), William Derham (1657-1735), François Fénelon (1651-1715), William Paley (1743-1805) and William Wollaston (1660-1724) in natural theology. Indeed, he tackled his topic as persuasively and elegantly as any of them.

The analysis of power began in part two of the treatise, entitled 'Showing the application of the living forces'.[16] Having received the purely mechanical operations of living forces, he set out to show how the origins of living motions were different from those of mechanical forces, and how the former exhibited power and design far superior to anything predictable from the mere mechanical adjustments of living parts. Bell asserted that the connexion between vital and inorganic powers was nought, that even the various forms of vital power were not interconvertible or correlated with one another, and that though there was an inscrutable sympathy among living tissues, vitality could not be transferred from one part to another. These ideas were put most succinctly in a comparison between a living system and a jackstone:[17]

> "The jackstone produces motion in one part of a machine; that, varied by mechanical influence, is communicated to a second; .... But what a base notion it is to suppose that the mere property of weight in the jackstone is like the influence of life!
>
> The weight is the power, in the language of mechanicians; but it does not reside in the parts of the machine, nor does it exhibit different qualifications in these parts. Separate them, and they are nothing. On the contrary, no one part of an animal body is in this manner dependent on another for its property of life. The property is inherent in the part itself, and the wonderful thing is that each property in the several organs corresponds with the others, so as to form a circle of vital operations. There is no transmission of power, in all this, from part

> to part - no train of connection to be traced as from the jackstone or the spring along the parts of the machine. There is therefore, in truth, no resemblance between machinery and the influences in operation in a living body".[18]

Bell had utter contempt for all hypotheses proposing an analogy between mere mechanism and life; the useful philosopher then became, in his opinion, "a very indifferent physiologist".[19] The fashionable galvanic hypotheses came equally mercilessly under his hammer; none of the powers that had been investigated outside of the vital oeconomy were to be countenanced. With parts of his argument many contemporaries must have agreed; for instance, how true it is that:

> "Whatever notions have prevailed in the schools at different epochs, of heat, electricity, or galvanism, we find an attempt to explain the phenomena of life by an application of the powers, with which they have been successful in their physical inquiries".[20]

Besides, he had the authority of Newton and most British philosophers from Locke onwards to support his cry:

> "We here reach the limit of philosophical inquiry. Hitherto, all has been flattering to the pride of the creature; but we must now humbly acknowledge the inscrutable ways of the Creator; and ceasing to trace the origin of life, more than we do that of gravitation, we should be occupied in observing its laws, not in exploring its source".[21]

Like most physiologists, Bell was fascinated by the continuous wearing away and restoration of the body. This cycle of life and death of bodily parts was envisaged in two ways. Almost all physiologists in the first few decades of the 19th century envisaged a material cycle, whereby the anatomical frame of the organism was conserved. But by the 1830s, a few physiologists also thought of a dynamic cycle, whereby the body's powers, as well as its particles, were conserved. By the 1840s, some thoroughly sober physiologists were holding the dynamical view in preference to the material one - much as Brown had done, though with much more reliance on empirical and experimental evidence. To this category belonged William Carpenter, Peter Roget and John William Draper of the British school, and Julius Mayer and Hermann Helmholtz of the German one.

Bell belonged to the first category, for although power was a key idea in his physiology, it was not used in his view of the wearing-down and restoration of the organism. In this sense, T.S. Hall's assertion[22] that, even more than the marvellous architecture of the body, it was the intimate dynamics of vital processes that convinced Bell of the continuance of God's creative power and guidance, needs to be qualified. Bell, it seems to me, was interested more in structures and movements than in true dynamics, when seeking God in physiology; indeed, his view was that of an anatomical architect, thinking of pillars and kingposts (his analogues of hollow bones) and the foundation stones of the Eddystone lighthouse (his analogue of the bone structure of the human foot), rather than of the preservation of power in the vital oeconomy. The same architectural spirit informed his other works in natural theology, namely his Bridgewater treatise on *The hand* (1832)[23] and his *Essays* (1841-1842)[24] which were meant as appendices to Paley's *Natural theology.*

Marshall Hall a colleague of Bell and an equally renowned neuro-anatomist and physiologist, also published a treatise on blood circulation; the manuscript was written by 1831 but published only in 1844 as *A critical and experimental essay on the circulation of the blood.*[25] Like Bell, Hall was interested in the powers moving and retarding the blood; he also tackled the phenomena of 'sympathy'. Unlike Bell, he did not think that vital effects could proceed from insufficient causes, and though emphasizing the unique, vital nature of the animal oeconomy, he did not condemn attempts to find correlations between its powers and those of inorganic nature.

On the mutual connections among the vital powers, particularly vis-à-vis the heart and circulation, Hall used Darwin's model of intertwining rings. The model illustrated how there was a mutual connection among the living parts, each part (or ring) supporting others, and the health of any one affecting the others. The three main rings in Hall's scheme represented the nervous, vascular and organic circles.[26]

Throughout the essay Hall seemed to be seeking a semi-quantitative expression of the interrelations of these circles. He thought he succeeded in a paper he read to the Royal Society of London in 1832, concerning a 'Theory of the inverse ratio which subsists between the respiration and irritability in the animal kingdom'.[27] There, he tried to give rigour to his dynamical physiology, and in so doing he hinted at a tight connexion - even a correlation (though he did not use that word) - between the powers of inorganic nature, (namely the chemical powers in respired air), and the powers of organic nature, (in the forms of irritability and contractility). In some features, his theory was reminiscent of Brown's, but it was considerably more scientific in its reliance on an extensive survey of comparative physiology, whose range and thoroughness reminds one of W.F. Edwards' treatise *De l'influence des agents physiques sur les phénomènes de la vie* (1829),[28] which in the 1830s and 1840s was a classic in both British and Continental comparative physiology. In fact, Hall cited "the extraordinary works of LEGALLOIS and M. EDWARDS".

Hall's theory, which he considered of seminal importance, asserted that the exhaustion and restoration of irrito-contractility depends critically on the quantity of stimulus which the organism receives; that excessive stimulus uses up irrito-contractility, and vital activity is dependent on, or even correlated with, physico-chemical stimuli - for instance, he wrote that vital activity "... is the pure effect of high stimulus";[29] that when irrito-contractility is exhausted, rest is required to restore it - therefore, the animal oeconomy was not considered a *perpetuum mobile*, although Hall did not enquire into the source of restoration of the irrito-contractility; and that similarly quantitative relations between organic and inorganic power would be found for other vital functions, in addition to muscle activity.

He aimed

> "... to trace a peculiar law in the animal economy, through the various series, forms and conditions of animated being. This law may be announced in the following terms: The quantity of the respiration is inversely as the degree of the irritability of the muscular fibre".[30]

He did not mean a strict mathematical relation, simply that when respiration is great, the degree of irritability is low, and vice versa. He envisaged life as a purely dynamic balance, resulting from stimuli impinging on parts endued with irritability, the principal stimuli being air, food and heat, the principal and corresponding organs of irritability being the heart, stomach and general muscular system. Throughout the animal kingdom, this inverse ratio of respiration to stimulus obtained; birds and mammals possessed great respiration but low muscular irritability, whilst reptiles, batrachia and fish possessed little respiration but high irritability.

Hall was a fine experimentalist and took great care in constructing his instruments to measure respiration and irritability. He was also cautious in drawing conclusions, but so impressed was he by the broad inductive basis of this investigation that he proposed his inverse ratio law as the most important one for preserving life;

it appeared to him "to constitute a chain which links together all the phenomena of the animal economy. I believe it to be the most general and inclusive in physiology".[31]

Like Bostock, Hall would surely not have been surprised at the physiological applications of the correlation and conservation of forces, made by Fowler and Carpenter in the late 1840s. However, I have not yet found any comments by him on their work.

William Pulteney Alison studied medicine at Edinburgh, where he became a keen follower of Dugald Stewart who wanted him as his successor in the chair of moral philosophy. However, he remained in medicine and became an enormously popular and respected figure in Edinburgh medicine. As a physician to the New Town Dispensary he undertook a special study of fevers, and his reports to the *Edinburgh Medical Journal* from 1817 to 1819 were important contributions to febriology; in those reports he also advocated Edward Jenner's (1749-1823) smallpox vaccination, which was still an unusual technique. From 1820 to 1855 Alison held medical chairs in Edinburgh and took a great interest in social conditions in Scotland, especially among the poor; his recommendations were largely embodied in the Poor Law of 1845. Alison is remembered today for his work in social medicine, but in his own day his *Outlines of physiology* was quite widely read and with that we are concerned in this chapter.

Comrie's assessment[32] of Alison's *Outlines* was that it was largely concerned with his ideas on vital attraction and repulsion, which he considered to be characteristics of life as exhibited by the tissues. This in itself gives no indication of the quality of his work, and for the purpose of this thesis we might notice the following features: Alison did not seek a correlation between organic and inorganic powers and he was not an experimentalist. Yet he merits discussion, because power was a central concept in his physiology, and perhaps more than any other writer of his time he realized that the paradigm of power was the main factor distinguishing the old physiology from the new. He believed that a true science of physiology had begun only when men like Cullen had focused on "The primary moving powers of the animal economy".[33] Moreover, several of his pupils went on to become solid physiologists in British medical schools and took his faith in dynamical physiology with them. One idea of his is especially interesting, namely that the chemical reactions between the body's tissues and the blood actually aid its circulation, and that these reactions constitute a *vis-à-fronte* which draws the blood along. This idea was totally opposed to Bell's view of the circulation (see above), but it was taken up by John William Draper in America and incorporated into his rather idiosyncratic and wholly dynamical physiology (see chapter 8).

Despite his firmly vitalist philosophy, Alison never used vital force as an explanatory device in physiology; he was what T.S. Hall calls "a vital materialist",[34] in that the uniqueness of life simply seemed to be the logical induction that he could make from surveying certain combinations of chemical elements. He used his vitalism cautiously; for instance, having mentioned the apparent irreducibility of many organic phenomena, he concluded that

> "... In so far as we can ascertain this to be the case, we can say that these phenomena are effects of the Vital Principle, or of Vitality; and that is our definition of these terms. They are the general expression for those of the changes occurring in living bodies, which we judge to be peculiar to them; and stand in the same relation to the science of physiology, as the terms chemical affinity, electricity, gravitation, to other departments of physical science".[35]

Moreover,

> "Of such first principles in science, we can give no other account, than that they depend on the Will of the Author of Nature".[36]

Though Alison did not explicitly champion a single, all-encompassing vital force, he came close to championing vital affinity - the organic analogue of chemical affinity - for that office. In his *Outlines of physiology* he proposed that when blood enters the capillary vessels of living tissue, it comes under the laws by which movements of nutritive fluids occur in both animals and vegetables, in addition to the propulsive power of the heart and arteries. Those laws were due to vital affinities, which curiously led him to a most avitalistic-sounding mechanism for blood circulation, namely, that when the chemical changes between air and blood are arrested in the lungs, the heart and arteries fail; the circulation therefore had at its root a chemical process.[37]

Vital affinity, as well as the other powers of the animal economy, were discussed at even greater length in his *Outlines of physiology and pathology* (1833). There too, vital affinity was exalted, albeit cautiously, as the most fundamental organic power:

> "Its existence will always be an ultimate fact in physiology; but the limits of its agency, and the laws according to which it modifies the chemical relations of substances subjected to it, may be ascertained; and their development will probably constitute the next great discovery in this science".[38]

Alison thus believed the investigation of power to be the primary programme for physiology. For this he praised Haller, Cullen and John Hunter.

> "This principle was recognized in the favourite position of CULLEN, that medical science must be founded on a knowledge of the 'moving powers of the animal economy'. But I think it certain that Cullen misunderstood the nature of these moving powers in one essential particular, viz. in supposing that the vital power of muscular parts is essentially dependent on an influence derived from the brain".[39]

In Alison's opinion, the truth would be found by investigating the powers associated with the motions of the blood and other fluids, and not by investigating the nerves. Though a vitalist, Alison clearly hoped to discover connexions, perhaps even correlations, between vital and physico-chemical powers, and thus to treat the vital economy with more rigour than had been possible hitherto.

Alison's most interesting and fundamental discussion of power came in his chapter on the mental faculties. There, he considered power as the first step in natural theology, since it was truly an attribute of mind. In opposition, he cited Dr. Thomas Brown (1778-1820)[40] who had argued that no further idea could be attached to power than mere 'invariable antecedence', and that the notion of causality arose simply from appearances of contrivance or adaptation of means to ends. Brown's argument was clearly based on Hume. Alison thought that Dugald Stewart had answered that particular point, but that he had only partially redeemed power, for his view of natural philosophy had been that

> "... the only foundation of much of our belief, and the only source of much of our knowledge, is to be found in the constitution of own minds".[41]

Alison developed Stewart's view, by asserting that sense or perceptions were also important means for acquiring understanding, even of such an abstract agent as power. Hence, he admired both the scholastic maxim, '*Nihil in intellectu quod non fuerit in sensu*', and Leibniz's addition thereto '*Nisi intellectus ipse*'. Ultimately, just as Leibniz had, (see chapter 10 in this thesis), Alison based his belief in the reality of power on the Will of God.[42] If one believed in His Will - which Alison did, it would be fully meaningful to use the concept of power - which Alison and other physiologists were doing, and the great goal of science would be to understand the laws, perhaps even the very nature, of power - which Alison called 'the first step in natural theology'. On the other hand, if one was aware of the difficulties in the interpretation of sense-perceptions and the acquisition of knowledge generally, which Alison was, one would appreciate the magnitude of man's uncertainty *de rerum natura*: this was why the philosophers were quoted so often by Alison and his fellow physiologists when they wrote upon the senses. Clearly, Alison and his contemporaries believed that the pure sciences of physiology, physics and chemistry could never give them understanding of sense-perception, unless the insights of pure philosophy were also employed. The British physiologists were not alone in this belief. At the same time, similar ideas were alive in Germany; for instance, a brilliant young physiologist in Bonn was investigating sense-perceptions experimentally as well as philosophically; his conclusions were to be seminal and were, in the opinion of one of his future pupils, to lay the foundation for a new era in physiology. That young physiologist was Johannes Müller; his pupil was Hermann Helmholtz. They both contributed greatly to the physiological discussion of power.

Neither Alison nor his pupils developed the study of sense perceptions, particularly vis-à-vis its philosophical consequences, in the magnificent way that the Germans did. However, John William Draper, in America, was impressed by Alison's pointers for future physiological research and developed the dynamical study of the blood and the sap of plants. That was one of the most fruitful consequences of Alison's work.

<u>Thomas Southwood Smith</u> studied first for the ministry, with the encouragement of William Blake (1773-1821), the poet and social reformer. In 1812, he began medical studies at Edinburgh; he practised as a Unitarian minister there until his graduation in 1816 when he moved to London. In London he busied himself in medical practice and natural philosophy, was one of the projectors of the *Westminster Review*, was appointed physician to the London Fever Hospital, and was a founding member of the Society for the Diffusion of Useful Knowledge. For the Society he wrote several treatises and contributed articles on anatomy, physiology and medicine to its *Penny cyclopaedia*. He became an ardent follower of Jeremy Bentham (1748-1832), and after Bentham's death he took an active part in the social reform movement. For the next thirty years, he devoted himself to philanthropic works, particularly the investigation of sanitary conditions, writing reports on epidemics and the promotion of the General Board of Health. He was clearly following the same steps as his Edinburgh contemporary, Alison.

In the 1830s, Smith published some excellent physiological works, two of which will be discussed here: *The philosophy of health; or an exposition of the physical and mental constitution of man, with a view to the promotion of human longevity and happiness* (1836),[43] and *Animal physiology* (1838),[44] which appeared in the same volume of *The library of useful knowledge* as Charles Bell's *Animal mechanics*. Between the mid 1830s and 1860, his public health work left him no time to continue his study of physiology and natural philosophy, and it was not until 1865 that a revised edition of *Philosophy of health* appeared. With that edition, too, we will be concerned.

The primary concept in *Animal physiology* was clearly force or power. The vital economy differed from inorganic ones primarily by virtue of its dynamical qualities, for it consisted essentially of circles of action and reaction, an idea akin to Erasmus Darwin's and Marshall Hall's.

Like several of his colleagues, Smith used the words 'vitality' and 'vital economy' without envisaging any special vital force; such words merely expressed the manifest distinction between life and non-life. The main distinction concerned the generation of power. In his section on the properties of primitive tissues he contrasted primitive cellular tissue, which had the purely passive power of elasticity, against primitive muscle tissue, which had its own vital source of power, namely contractility; he also compared inorganic mechanism, in which power could never be generated, with organic mechanism, in which power could be generated. Muscle, with its property of contractility,

> "... is the generator of power. It is not only not a mechanical property, but it possesses nothing in the slightest degree analogous to any mechanical force. In the best contrived machinery there is no real generation of power: there is merely an application of pre-existing power to some specific object. In the reaction of an elastic body, in the recoil of a spring, there appears to be an actual production of power; but the effect thus apparently produced is the mere reaction of the force originally employed in compressing the spring. The force of the recoil can never be greater than the force employed to compress it, and the

> moment this power is expended, all capacity of motion is at an end. In muscular contraction, on the contrary, there is a real generation of power. If a heart be pricked gently with a needle, the ventricle will instantly contract with such force as to propel the needle deeply into its substance .... There is thus an actual production of power, because the effect bears no proportion to its mechanical cause. There is, then, not only no identity, but no analogy, between this power and any of the great principles of nature, which are the original sources of mechanical force. And of this, the complete proof is that its most powerful effects are produced without the intervention of any mechanical cause - by an agent which has no relation to any physical property of matter, namely by volition. This power, therefore, is distinct from any other in nature and is peculiar to life".[45]

Smith attributed the peculiarity of life not to a special vital force, but to its construction by God, the grand mechanician. Like William Paley, the philosopher and theologian, he was at pains to assert that the living system is a superb piece of mechanism, but a mechanism of such stupendous skill and power, that it could only be accounted for by reference to a superb designer. Vital force could have no place, for Smith, in such a machine, for it would undermine its dependence on its designer for the harmony, beauty and power of its functions.[46] Vital force as an ultimate cause of motion was therefore rejected as a pagan concept, and the primary purpose of his *Animal physiology* was to deal a death-blow to that concept.

Smith was not the only medical theorist of his time to consider the theological implications of vital force. As T.S. Hall has mentioned,[47] these implications were being considered by several British and Continental writers; in Britain, Hall mentioned James Cowles Prichard (1786-1848) who wrote an incisive and useful work called *A review of the doctrine of a vital principle* (1829);[48] Prichard concluded that the basic functions of life are merely the effects of organization of matter, whilst the processes of embryonic development are too complex and wonderful to be attributed to any agency save the Deity. (Prichard will be discussed in the chapter on Johann Friedrich Blumenbach (1752-1840), for he was a principal presenter of Blumenbach's theory of the 'Bildungstrieb' to the English medical world, and his ideas are best discussed in the light of Blumenbach's).

Although refuting vital force as a distinct entity superimposed on matter, Smith denied that the idea of non-creatibility of power in inorganic mechanism could apply to the living machine. Nonetheless, he did not believe that the machine operated or generated power at random, for that would destroy any hope of a rational science of physiology. He 'saved the phenomena' by envisaging the organism as a set of circles of actions and reactions, so that whatever happened in one part necessitated a specific, and hopefully quantifiable, change in another.

> "A circle of actions is established within the body, by which certain processes are accomplished which counteract decomposition

> and retain it in the peculiar condition which, as far as our observations go, is necessary to life. Thus, internal, or as they are often termed, intestinal actions, counteracting the ordinary action of physical agents, afford the first distinctive character of life".[49]

In Smith's opinion, the main task of physiology was to account for that circle of actions. This task was also tackled in his *Philosophy of health*, but in a different context, for this treatise had been commissioned by The Society for the Diffusion of Useful Knowledge and, in keeping with the spirit of the society, Smith aimed to illustrate the theological conclusions to be drawn from physiology. This had also been Bell's brief for his essay on animal mechanics. *Philosophy of health* was also intended as a physiological contribution to Benthamism. Bentham had wanted Smith to lead his movement after his death, and the introduction to the book reflected Smith's Benthamite allegiance as well as the vision for science that men like Beddoes and Davy had held - namely, that a knowledge of the human constitution would benefit society in general, and was indispensable to "the barrister, the judge, the magistrate and the legislator".[50] Actually, Bentham himself was greatly interested in medicine, especially the organization of medicine, as Benjamin Spector has discussed in a recent paper.[51] He even left his body to medical research.

As in *Animal physiology*, the primary concept in the *Philosophy* was power. There, Smith emphasized not only the circles of action and reaction, but also the dependence of organic functions on external, inorganic powers. He seemed to seek a fundamental connexion between organic and inorganic powers, whose conjoined operation would constitute life. The inorganic agents were primarily air, water, heat, cold, electricity and light; and

> "Without the living organ, the physical agent can excite no vital action: without the physical agent, the living organ can carry on no vital process. The plant cannot perform the vital process of respiration without the leaf, nor with the leaf without the air. The physical agent acts upon the living organ; the living organ reacts upon the physical agent, and the action between both is definite. ...In this manner, the change in the physical agent is definite and uniform; and the change in the living substance is equally definite and uniform".[52]

> "It is this determinate interchange of action between the living organ and the physical agent that constitutes what is termed a vital process".[53]

Indeed, the totality of an organism's vital phenomena comprised the changes that living organs and physical agents effected upon one another. Physical and chemical attractions were supposed to be "controlled and modified" and converted into the dynamical or material basis of life. Although the creation of organic power *de novo* was not denied explicitly, that denial seems implicit in Smith's argument, at least with regard to the 'organic' functions, that is all the functions and powers of plants and the 'involuntary' functions and powers of animals. Animals possessed another category of functions, 'animal functions', to which the afore-mentioned dynamical laws did not apply. Thus, in both *Animal physiology* and *Philosophy of health*, Smith differentiated between animal powers, which appeared to be *sui generis*, and the inorganic, purely mechanical powers which he considered to be subject to neither creation nor annihilation.

> "The force exerted by the heart is vital. It is distinguished from mechanical force, in being produced by the very engine that exerts it. In the best constructed machinery there is no real generation of power. There is merely concentration and direction of it. In the recoil of the spring, in the reaction of condensed steam, the energy of the expansive impulse is never greater than the force employed to compress or condense, and the moment this power is expended, all capacity of motion is at an end. But the heart produces a force equal to a pressure of 60 lbs. by the greatest application of a bland fluid. Here, no force is communicated, to be given out again, as in every mechanical moving power; but it is new power, power really and properly generated; and this power is the result of vital action, and is never in any case the result of action that is not vital".[54]

Smith was acquainted with several careful, experimental studies on the blood circulation; he cited Stephen Hales, Albrecht von Haller, the Abbé Spallanzani (1729-1799), John Hunter and Jean Poiseuille (1799-1869); consequently, his belief in self-moving powers in the animal economy was supported by a thorough review of the topic; like Bell and John Hunter, he saw no escape from admitting that blood is alive.

Smith's vital economy differed from Bell's, in that he insisted on a tight interdependence of its several circles of power. Whereas Bell asserted that vital powers, though sympathetic to each other's levels of activity, were essentially independent and could not be transformed into any other form of power, Smith believed the transformation or correlation of vital powers to be the cornerstone of physiology. He wrote of three great centres or circles of power, namely respiration, circulation of the blood, and the nervous system;[55] of organic life, the lungs and heart were the primary seats; of the animal, the brain and spinal cord. Between them all, the bond was so close that any lesion of one affected another, and none could exist without the support of the others. Indeed, "... they form a triple chain, the breaking of a single link of which destroys the whole".[56]

Such ideas were not new.[57] But what was fairly unusual was Smith's discussion of respiration as the most essential of the three dynamical centres; to discern the relation of respiration to the other functions was to take the most comprehensive view of the economy. Indeed, he seemed to imply that even the animal powers depend on a prior, inorganic source of power, namely, the chemical energy entailed in respiration, and that the heart and muscles do not possess power *sui generis* and do not constitute a *perpetuum mobile*. Thus,

> "The first and most important use of the function of respiration is to maintain the action of the organs of the animal life. It has been shown (vol.1, ch.2) that the

organic is subservient to the animal life, and that to build up the apparatus of the latter, and to maintain it in a condition fit for performing its functions, is the final end of the former".[58]

Such support of the animal functions by the organic was, of course, easy to envisage as a material process, whereby the air and blood contribute the particles out of which the body is constructed. Smith held this material view of the body's maintenance, but also a dynamical view whereby respiration "endows the blood with the power of maintaining the contractility of the muscular fibre".[59] This dynamical view was supported by experimental studies which indicated that an animal's heat was directly proportional to the quantity of air which reacted with the blood.

One important consequence of his view of respiration was that it suggested a fundamental connexion, even a dependence, between vital and inorganic powers, and that physiologists should seek quantitative expressions of that connexion. Already, the quantitative study of respiration by Dulong and Despretz had shown that c.80% of an animal's heat was attributable to chemical energy in its food and inspired air, and it was a clarion call to experimental physiologists to see whether there might be a total correlation. Though Smith did not contribute any experimental research to that issue, he realized how important it was; in his preface to *Philosophy of health*, he wrote that by studying the effects of air, water, heat, light and electricity on organized bodies, the physiologist would deduce the rules for the management of food, clothing, sleep, exercise and all other agents which affect man's physical and intellectual powers and their due balance. All this had a grand, practical end in view:

> "The investigation, in this manner, of the properties and powers of the entire organism, in its relation to external nature, can alone afford in a sense satisfactory and useful to the philosopher, the educator and the legislator, a knowledge of human nature".[60]

The investigation of the organism's dynamic relationship with the outside world was therefore of paramount import, not solely for medical science but also for ethical and socio-political sciences. Hence, he promised his readers a full account of the action of physical agents on the living organism, with particular regard to the development of the physical and mental powers of man. Clearly, Smith had imbibed Bentham's teachings thoroughly. As Spector's paper shows, Bentham had had a long interest in medicine, in the way it was organized, taught and fitted into his philosophy of maximum happiness and utility. Bentham had asserted that to understand and ameliorate human society, one had first to understand the human organism. Smith remained true to that Benthamite ideal throughout his career, as evidenced by his physiological writings.

A third volume of *Philosophy of health* had been intended from its inception, to discuss the nervous and muscular systems. Not until the 1860s, for the eleventh edition, did Smith find time to compose it; he died before the new edition went to press, so that its arrangement was handled by his grandson. The new edition did not deviate essentially from the first; again, the heart and artery walls were generators of their own power, though it was emphasized repeatedly that their activity was never haphazard and depended always on the action of external, inorganic agents. Indeed, it is difficult to determine exactly how vital the organic powers were thought to be, for their dependence on inorganic agents had become even greater than in earlier editions.

> "The exciting bodies are called stimulants, and the actions they produce, since they take place only in organized and living structures, are called vital. Without the structure, without the stimulus, there is no action: both must combine and co-operate. Life consists of vital actions, sustained in organized structures by the excitement of stimulants".[61]

Despite the new material in the eleventh edition of Smith's *Philosophy*, one finds no basic physiological premises which were not in the first. This was partly because Smith was now too old to change the ideas of a lifetime; he did not mention Liebig, Helmholtz or a number of other workers who had contributed meanwhile to experimental physiology. It might also have been because the dynamical physiology that he had championed in the 1830s seemed to him to be holding up so well. Indeed, the *Philosophy*, which had been so up to date and stimulating at its first publication, was still a useful text in 1865, and one of its principal aims was still to further the investigation of power, in both its organic and inorganic forms. Therein, in his opinion, lay the future of physiology and natural philosophy.

<u>Thomas Bateman</u>. One of the most comprehensive and highly regarded of the British encyclopaedias during the first half of the 19th century was *Ree's cyclopaedia*, or *The new cyclopaedia or universal dictionary of arts and sciences*.[62] The first volume appeared in 1802; it was completed in forty-six volumes, plus six volumes of plates, in 1820. Abraham Rees, D.D., (1743-1825) persuaded a host of experts to contribute articles on their own subjects; consequently the standard of many entries was remarkably high. The entry on fever was written by a London physician, Thomas Bateman, who, after studying in Edinburgh, moved to London and became a physician at the Fever Institution. In London he had worked under Robert Willan (1757-1812), whose speciality was skin disease; in 1819 Bateman made Willan's work known in a highly regarded treatise called *Practical synopsis of cutaneous diseases according to the arrangement of Dr. Willan*.[63] Of all Bateman's writings, his article on fever was the most penetrating and discursive on physiological theory, and it will be his only work discussed here. It was a remarkable article in that it was exceptionally long for an encyclopaedia - 62 quarto double-columned pages.[64] It was crammed with historical and current data on the theory and treatment of fever, and must have impressed its medical readers for it was cited frequently in physiological treatises throughout the 1820s and 1830s. It merits discussion in this thesis, for it corroborates several assertions I have been making about late 18th and early 19th century physiologies, in particular the importance of power.

Early in his article, Bateman made a simple point about the symptoms of fever, namely that these have always been recognized as abnormal degrees of heat and cold, shivering and augmented pulse.[65] The most conspicuous of these was the heat; hence the word πυςετοσ, from πυς, fire, had been the Greek appellation for fever, and 'febris' had been the Latin one. If one agrees with this discussion of fever symptoms,[66] one would surely agree that fever must always have been difficult to account for in terms of static agents, or at least that it would always have been amenable to quasi-dynamical explanation. Thus, once the concept of power or force had been given a reputable experimental and philosophical foundation (by Newton, Locke and the post-Newtonian philosophers with the exception of Hume), and the humo~~u~~ral, iatro-chemical and iatro-mechanical hypotheses were found wanting, fever would have become an obvious candidate for the application of dynamics. This is borne out by the fact that the only 18th century theories of fever that Bateman considered worth detailed discussion were all dynamical.

Bateman thought of fever as the immediate consequence of a disturbance of nervous power, such disturbance having repercussions on all the other functions and powers of the vital economy. The symptoms of the onset of fever, namely the cold stage, were due to diminution of nervous energy; the symptoms of the later hot stage were due to a recoiling of the nervous power.[67] Nervous power was the primary form, with which all other powers of the vital economy were correlated.

> "We cannot think much and use strong exercise at the same moment. Both these powers, therefore, seem to depend alike on the nervous energy, and the simultaneous diminution of both implies the diminished state of that energy".[68]

The question of what caused the disturbance of nervous power, what was the proximate cause of fever, could not be answered unequivocally. Rejecting humoural pathology and Cullen's contagion and miasm hypothesis for idiopathic fevers, Bateman thought that three recent dynamical hypotheses had come close to the truth; these had focussed on the functions and powers of the vital economy, particularly the sensorial powers; they had taken their cue, in his opinion, from the ideas of Thomas Willis (1621-1675), Friedrich Hoffmann (1660-1742), and especially from the 4th edition of Boerhaave's *Aphorisms* where he had introduced the 'nervous fluid' which, in communicating a sluggishness to arterial blood, constituted a proximate cause of intermittent fevers. Bateman wrote that since Boerhaave's day,

> "... the phenomena resulting from the sensorial functions in disease became the subject of more attentive investigation, and hence have arisen 3 modern systems, in which the nature of fever is explained by a reference to these functions .... These are the theories of Cullen, Brown and Darwin. The theory which Dr. Cullen promulgated ... almost rivalled that of Boerhaave in the extent of its reception; and even continues to be adopted by some later writers. ... The Brunonian fever theory is not particularly prominent in the general system of its author. ... That of Dr. Darwin is more complex, but much more comprehensive in its application to the

> varieties of fever; it is obscure, however, from the peculiar language in which it is expressed".[69]

Darwin's theory, like Cullen's and Brown's, depended on excitability, but he added the idea of association or sympathy, thus calling it the 'sympathetic theory of fever'. Bateman considered the two most useful and general laws that Darwin and Brown had enunciated for physiology to be that i) All excitement or activity of an organism occasions a diminution of its power, ('excitability' in Brown's terminology, 'sensorial power' in Darwin's), according to the degree of the excitement; and ii) Rest or abstraction of the usual stimuli render the organism more susceptible to stimuli subsequently applied.[70] In these two laws there was clearly the idea that the vital economy, like purely mechanical systems, required the restoration of its susceptibility to action, what we would call its energy, before it could resume its activity; and although Bateman did not inquire into the source of that restoration in Darwin's and Brown's theories, he did seem to think it not *sui generis*. If he had believed that vital power could be created *de novo* whenever it was needed, there surely would have been no sense in his quoting Darwin on the principle that the torpor produced in an organism, as a result of its activity, was more intense or of longer duration, in proportion to its expenditure of sensorial power.[71] A wholly random generation of power would also have been incompatible with the tenor of Darwin's third law of physiology, which Bateman described approvingly:

> "That the functions of different parts of the system are so far catenated, or associated with each other, as it were in circles, either from direct connection in structure, or from the habit of acting together, or more frequently from causes at present inscrutable (see 'Catenation'), that an increase or decrease of the action of one organ is followed or accompanied by an increase or decrease of the action of another ...."[72]

The treatments that Bateman advocated, on the basis of the dynamic theories of fever, were themselvs entirely dynamic; only scant attention was paid to chemistry. He proposed that, whatever theory of disease is adopted, the two principal aims should be to diminish those activities and powers which are in excess, and to increase those which are defective.[73] In the language of Cullen, the first aim was "to moderate the violence of reaction", and the second was "to remove the causes or obviate the effects of debility". Clearly, Bateman's and many other physiologists' fever therapies rested upon the physics and dynamics, the balancing of forces, of the animal economy, a balancing act which presupposed a fundamental, and hopefully one day quantifiable, connexion between organic and inorganic powers.

Bateman's theory of life, and thus his theory of fever, employed the paradigm of power or force, for only such an agent could be versatile enough to account for the ever changing and intricate conditions of health and disease. Chemistry was simply too crude and limited to construct a fever theory, as we see in a passage where he discussed the use of emetics and purgatives in removing putrescent matter from the intestines:

> "With a view to correct the putrescency of these contents of the alimentary canal, various antiseptics have been recommended, especially the mineral acids. That these acids, especially the vitriolic ... are sometimes useful, cannot perhaps be questioned; but it is probable that their administration was suggested upon the principle of being *chemically* antiseptic to dead animal matter. A chemical explanation of the operation of a medicine on the *living* body is somewhat suspicious; and Dr. Fordyce[74] has remarked that no antiseptic can be applied in that proportion to the living solid, as would be requisite to prevent putrefaction in dead animal matter".[75]

Summary

In this chapter, the physiological theories of six practising and well-known physicians of the first half of the 19th century in Britain have been discussed. They all believed power to play a central rôle, both within the vital economy itself and in its reactions to Nature at large. With one exception, Sir Charles Bell, they believed that the science of physiology would be established upon the quantitative analysis of organic and inorganic powers, and they suspected that correlations, or at least intimate connexions, existed among those powers. Sometimes they revealed that the non-creatibility of power had become a general principle in inorganic physics and chemistry. Occasionally, that principle also gained admittance, through a back-door as it were, to their discussions on the vital economy, but their constant reluctance to theorize too freely stopped it from being admitted explicitly. Power or force was their principal, and usually exclusive, physiological paradigm, for they believed that the essential difference between life and non-life lay somewhere in their dynamical differences. By and large, the humours, chemistry and even the vital force as a distinct agent in its own right were disregarded by them.

This interpretation of their theories, and their similarity to the earlier dynamical theories of Cullen, Brown and Darwin, are supported, I believe by Bateman's article on fever in *Rees's cyclopaedia*. My interpretation also agrees with other historians' studies[76] of 18^{th} and early 19^{th} century concepts of fever.

NOTES TO CHAPTER 5.

1. J. Bostock, *Essay on respiration*, Liverpool, J.McCreery for Longman & Rees, 1804.

2. J. Bostock, *Elementary system of physiology*, London, Baldwin, vol.1, 1824. vol.2, 1826, vol.3, 1827.

3. *Ibid.*, preface, p.iii.

4. N.G. Coley, *From animal chemistry to biochemistry*, Amersham, Hulton Educational, 1973, pp.86-87.

5. *Op.cit.*, Note 2 above, pp.160-161.

6. *Ibid.*, p.214.

7. *Ibid.*, p.218.

8. *Ibid.*, p.324.

9. *Ibid.*, pp.195-196.

10. My knowledge of Bell's life work is very limited, and I have relied greatly on secondary sources, the two most useful having been: i) Sir G. Gordon-Taylor & E.W. Walls, *Sir Charles Bell: his life and times*, Edinburgh, E. & S. Livingstone, 1958. And ii) P.F. Cranefield, *The way in and the way out ... with a facsimile of Charles Bell's annotated copy of his Idea of a new anatomy of the brain*, New York, Futura, 1974.

11. C. Bell, *An essay on the forces which circulate the blood*, London, Longman, 1819.

12. Sir Charles Bell, *Animal mechanics, or proofs of design in the animal frame*, Library of useful knowledge, iv, London, Baldwin & Cradock, 1838.

13. *Op.cit.*, Note 11 above, p.12.

14. *Ibid.*, p.23.

15. *Ibid.*, p.83.

16. *Op.cit.*, Note 12 above, pp.33ff.

17. I have been unable to find any exact technical meaning of jackstone that Bell might have had in mind. The dictionaries and cyclopaedias of his time define jackstone as modern dictionaries do, namely, as a small, smooth pebble or a small, specially-shaped bit of metal which is used in the game of jacks or dibs. Bell might have been thinking of the momentum or force that a jackstone possesses as it falls.

18. *Ibid.*, p.47.

19. *Ibid.*, p.50.

20. *Ibid.*, p.49.

21. *Ibid.*, p.48.

22. T.S. Hall, *Ideas of life and matter*, Chicago, Chicago Univ. Press, 1969, vol.2, p.231.

23. Sir Charles Bell, *The hand, its mechanism and vital endowments, as evincing design, Bridgewater Treatise iv*, London, W. Pickering, 1832.

24. Sir Charles Bell, *Practical essays*, Edinburgh, Maclachlan Stewart, 1841 and 1842.

25. M. Hall, *A critical and experimental essay on the circulation of the blood*, London, Sherwood, Gilbert & Piper, 1844.

26. *Ibid.*, p.158.

27. M. Hall, *Theory of the inverse ratio which subsists between the respiration and irritability in the animal kingdom*, read to the Royal Society, February 1832, published London, R. Taylor, 1832.

28. W.F. Edwards, *De l'influence des agents physiques sur la vie*, Paris, Crochard, 1824. This remarkable book went through several editions, was translated into English in 1832, and was cited extensively during the 1830s and 1840s. Regretably, since my space has been limited, I have had to omit any discussion of it, although it does bear upon my thesis.

29. *Op.cit.*, Note 27 above, p.11.

30. *Ibid.*, p.1.

31. *Ibid.*, p.14.

32. J.D. Comrie, *History of Scottish medicine*, London, Baillière, 1932, vol.2, p.611.

33. W.P. Alison, *Outlines of physiology and pathology*, Edinburgh, Blackwood, 1833, p.viii.

34. T.S. Hall, *op.cit.*, Note 22 above, pp.221 and elsewhere. The phrase 'vital materialism' was actually coined by Owsei Temkin.

35. W.P. Alison, *Outlines of physiology*, Edinburgh, Blackwood, 1831, p.2.

36. *Ibid.*, p.4.

37. *Ibid.*, his discussion on respiration, esp. p.201.

38. *Op.cit.*, Note 33 above, p.58.

39. *Ibid.*, p.viii.

40. Thomas Brown, the metaphysician, was cited occasionally by British physiologists between c1820 and 1850, but not often enough to merit inclusion in Chapter 1 of this thesis.

Brown studied at Edinburgh continuously from 1792 to 1803. At first, he read philosophy, logic and allied subjects and came under the influence of Dugald Stewart. In about 1796 he began to be interested in Erasmus Darwin's *Zoonomia*, on which he published some critical comments in 1798. From that year until 1803 he studied medicine, concluding with a dissertation '*De somno*'.

In 1804 he participated in a famous academic controversy at Edinburgh. The claim of John Leslie (1766-1832) to the mathematics chair was being opposed because he had supported Hume's theory of causality. Brown argued that Hume's theory did not lead necessarily to the sceptical conclusions usually drawn from it, and he published his argument *Observations on the nature and tendency of the doctrine of Mr. Hume concerning the relation of cause and effect* in 1804. A second edition appeared in 1806 and a third, enlarged one in 1818.

In 1808-1809 he began to assist Dugald Stewart in lecturing on moral philosophy, and in 1810 he was elected by the town council as Stewart's official colleague, a post that he held for the rest of his life.

Among his many published works, his lectures were probably the most popular and highly regarded, a nineteenth edition appearing as late as 1851. There, his inquiry into causality was one of the most vigorous statements of Hume's doctrine; like Hume, he reduced causality to mere invariable sequence and emphasized that 'power' expressed nothing but such sequence. He differed from Hume in asserting that man has an intuitive idea, subsuming all experience, that the same antecedents always produce the same consequents; this replaced Hume's 'custom' as the foundation of man's idea of causality and enabled him to avoid Hume's theological scepticism; Brown could happily admit God as the First Cause of an orderly universe.

Brown's philosophy was attacked sharply after his death, especially by Sir William Hamilton, the editor of the collected works of Thomas Reid and Dugald Stewart. (See Chapter 1). Brown was accused, with some justification, of merely taking the ideas of Reid and Stewart and adding some ideas of the French 'sensationalists' such as Condillac. Whatever Brown's originality, it must be admitted that he was the last, vigorous member of the Scottish philosophical school which had elaborated its own philosophy largely unaffected by the rising school in Germany. From then on, it would no longer be possible to ignore German metaphysics, particularly that of Kant and the *Naturphilosophen*.

I am aware of my own inadequate understanding of Brown's ideas and possible influence, and this is but one area of this thesis which requires further study.

41. *Op.cit.*, Note 33 above, p.218.

42. *Ibid.*, the whole of chapter 13, esp. p.216.

43. T. Southwood Smith, *The philosophy of health; or an exposition of the physical and mental constitution of man, with a view to the promotion of human longevity*

and happiness, London, G. Knight, vol.1, 1836, vol.2, 1837.

44. T. Southwood Smith, *Animal physiology*, *The library of useful knowledge*, iv, London, Baldwin & Cradock, 1838.

45. *Ibid.*, pp.21-22.

46. This is my interpretation, after having read the whole of his *Animal physiology*, even though I am well aware of how frequently he used expressions like vitality and vital force. Among those sections in *Animal physiology* which I think support my interpretation are the first two pages and the section on 'Course of the blood in the foetus', esp. p.78.

    On this irreconcilability between a truly vital force and an omnipotent and ever-acting God, see my section on Paley in Chapter 9.

47. T.S. Hall, *op.cit.*, Note 22 above, pp.231-239.

48. J.C. Prichard, *A review of the doctrine of a vital principle*, London, Sherwood, Gilbert & Piper, 1829.

49. *Op.cit.*, Note 44 above, p.6.

50. *Op.cit.*, Note 43 above, vol.1, p.vi.

51. B. Spector, 'Jeremy Bentham (1748-1832): his influence upon medical thought', *Bull.Hist.Med.*, 1963, 37:25-42. See also F.N.L. Poynter, 'Thomas Southwood Smith - the man', *Proc.Roy.Soc.Med.*, 1962, 55:381-391.

52. *Op.cit.*, Note 43 above, vol.1, pp.21-22.

53. *Ibid.*, p.23.

54. *Ibid.*, pp.390-391.

55. *Ibid.*, vol.2, pp.113-114.

56. *Ibid.*, p.114.

57. Indeed, they have been a continuous strand, in more or less vague form, in the preceding chapters. They are discussed most usefully in: E. Mendelsohn, *Heat and life: the development of the theory of animal heat*, Cambridge, Mass., Harvard Univ. Press, 1964. Also G.J. Goodfield, *The growth of scientific physiology*, London, Hutchinson, 1960.

58. Smith, *op.cit.*, Note 43 above, vol.2, p.115.

59. *Ibid.*, p.116.

60. *Ibid.*, preface to vol.2, p.xii.

61. T. Southwood Smith, *The philosophy of health; or, an exposition of the physiological and sanitary conditions conducive to human longevity and happiness*, 11th edition, revised and enlarged, London, Longman, Green, 1865, p.2.

62. A. Rees, *The cyclopaedia; or, universal dictionary of arts, sciences and literature*, in 39 volumes, London, Longman, Hurst, Rees, Orme & Brown, 1802-1820.

63. T. Bateman, *Practical synopsis of cutaneous diseases, according to the arrangement of Dr. Willan*, London, Longman, 1813.

64. *Op.cit.*, Note 62 above, vol.xiv, unpaginated.

65. *Ibid.*, pp.1 & 2 of the fever article.

66. Amongst the many studies that have been done on fever, I have found the following useful for discussions of its symptoms. i) H.M. Winans, 'Evolution of the concept of fever in the 19th century', *Ann.Med.Hist.*, 1935, N.S.<u>7</u>:27-35. ii) S.A. Gallacher, 'Stuff a cold and starve a fever', *Bull.Hist.Med.*, 1942, <u>11</u>:576-581. iii) P.H. Niebyl, 'Old

age, fever and the lamp metaphor', *J.Hist.Med.*, 1971, 26:351-368. iv) J.M. Eyler, 'William Farr on the cholera: the sanitarian's disease theory and the statistician's method', *J.Hist.Med.*, 1973, 28:79-100.

67. Bateman, *op.cit.*, Note 64 above, pp.10-12 of his fever article.

68. *Ibid.*, p.10, column 1.

69. *Ibid.*, p.22, column 1.

70. *Ibid.*, p.24, column 1.

71. *Ibid.*, p.26, column 1.

72. *Ibid.*, p.24, column 2.

73. *Ibid.*, p.27.

74. George Fordyce (1736-1802) was an eminent physician and teacher in London, whose literary output was quite extensive. Among the topics he studied and wrote upon were fever, his earliest treatise thereon being his *Dissertation on simple fever*, London, J. Johnson, 1794.

75. Bateman, *op.cit.*, Note 64 above, p.36, column 1.

76. In addition to the papers in *op.cit.*, Note 66 above, I have also found useful: R.A. Lewis, *Edwin Chadwick and the public health movement, 1832-1854*, London, Longmans, Green, 1952.

Chapter 6 Power in the natural philosophy of Peter Mark Roget (1779-1869)

The man is not wholly evil -
he has a Thesaurus in his cabin.

- Sir James Barrie describing
Captain Hook.

The one and only fact known widely today about Roget is that he compiled his *Thesaurus*. History, even history of science, has neglected all other aspects of his long and busy life, and with the exception of D.L. Emblen's biography[1] there has not been any attempt at a rounded appreciation of his work.

The *Dictionary of scientific biography* (1970) makes no mention of him. However, the *World who's who in science from antiquity to the present* gives him a fair mention[2] and the *Dictionary of national biography* gives him almost three pages.[3] He also has half a page in *Men of the reign* (1885).[4] Here I shall outline only the main features of his career, with the sole intention of showing that he was a well respected figure in London's scientific community from c1810 to 1850 and that his work in physiology and physics was widely read and well regarded even by his peers. What he wrote, therefore, - and I have his ideas on force or power particularly in mind - must have been at least an expression of up-to-date professional opinions; indeed, it will be argued that, though not an experimental genius of the order of Davy or Faraday, Roget was a man ahead of his times in some features of his physiology and physics and contributed original, sound, though not epoch-breaking, ideas of his own.

Roget began his medical studies in 1793 at Edinburgh. There, he was befriended by the professor of moral philosophy, Dugald Stewart, who was acquainted with Roget's uncle, Samual Romilly (1757-1818). Romilly had acquired a reputation for his critical writings on English political, legal and social institutions; he was soon to become a close collaborator of Jeremy Bentham and James Mill in the Law Reform Movement in England and he became the solicitor-general in the administration of 'All the Talents' of 1806-1807. Romilly helped Roget settle into the social and scientific circles when he moved to London in 1808.

Roget graduated in 1798 with a thesis on the laws of chemical affinity.[5] He spent the next year visiting several physicians and natural philosophers, calling on Erasmus Darwin at Derby and stopping a while with Thomas Beddoes and Humphrey Davy at Bristol. During this year he made contacts which were to be useful for his London career: Davy was one, and through Davy he met Davies Gilbert; Gilbert became president of the Royal Society in 1827 on Davy's retirement, and under him Roget became the society's senior secretary and editor of the *Philosophical Transactions*, posts he held for the next twenty-one years!

In 1800 Roget went to London and studied at Robert Willan's (1757-1812) dispensary and at St. George's Hospital under Matthew Baillie (1761-1823). Baillie had inherited the Great Windmill Street School of Anatomy from his uncle, William Hunter and was then one of the most eminent physicians in the country. After further peregrinations, Roget obtained his first professional appointment as physician to the

Manchester Public Infirmary, where his interests in public health and fever epidemics were first acquired. In Manchester he ran a course on physiology in the rooms of the Manchester Literary and Philosophical Society; in the preface to his course booklet[6] he declared his chief aim was to give 'organization' to that new science and to illustrate its 'relationship' with the other medical sciences. As Emblen mentions in his biography, organization and relationship were to be persistent aims in his life's work and, of course, became especially explicit in the *Thesaurus*.

He became a fellow of the Manchester Lit. and Phil. in 1805 and its vice-president for 1807-1808 when Thomas Henry (1734-1816) was president and John Dalton (1766-1844) secretary. But despite this auspicious start, Roget realized that the only place for an ambitious young physician and natural philosopher was London; accordingly, in 1808 he moved into a house in Bloomsbury, bought for him by Sir Samuel Romilly. He quickly established a prosperous practice, but he was not merely a respectable physician; he was soon writing medical and scientific articles for encyclopaedias, began lecturing at the Russell Institution in 1809, the Great Windmill Street School in 1810 and the Royal Institution in 1812; in short, he acquired a reputation as a fine lecturer and versatile natural philosopher; he also became known for fair judgement, as in his writings on phrenology.[7]

Among his activities soon after arriving in London was his work for the Medical and Chirurgical Society, which had been established recently to bridge the gap between physicians and surgeons. Roget collaborated especially with John Bostock (see chap.5), John Yelloly (1774-1842) and Alexander Marcet (1770-1822) in promoting the society. He collaborated too with Marcet's wife, Jane, working as her editorial assistant on her immensely popular book *Conversations on chemistry*.

In 1812 he began lecturing on physiology and comparative anatomy at the Royal Institution, with obvious success for he was asked to repeat his course in the two years following. In 1815 he began to contribute articles to the famous *Supplement* to the 4th, 5th and 6th editions of the *Encyclopaedia Britannica*, eventually writing over 300,000 words for it. Emblen discusses Roget's work for the *E.B.* in considerable detail.[8] Some of his articles were excellent; for instance, that on 'Bichat' in the 1967 edition was clearly a condensed version of Roget's original 1824 article. Glowing praise was given by the *Supplement*'s editor for his long article on physiology (1824):

> "Physiology is fully treated in the *Encyclopaedia*, in so far as concerns the principal facts relative to the functions of animal life; but there seemed to be wanting a comprehensive view of the general laws to which they are reducible; a defect which has been ably supplied by Dr. Roget".[9]

Roget considered his achievement in physiology had been to discover order and inter-relations among the vital functions, rather than the execution of new experiments. This accords with the interest he showed in Xavier Bichat (1771-1802), for Bichat's most important works, *Anatomie* *générale* (1801) and *Anatomie descriptive* (1802) had emphasized the usefulness of classification in the study of life. Emblen writes that one has only to recall Roget's own breakdown of the body's functions into neat classifications in his physiology course in Manchester, to realize that, like Bichat, he was persistently interested in applying categorical analysis to the animal economy.[10]

However, Roget was no mere compiler of other men's data. A certain degree of interpretative insight and creative thought is needed to review thoroughly, organize, and extract the general principles from any large collection of data. This was Roget's aim and none of his contemporaries, (except the phrenologists à propos his critique of phrenology), doubted how well he achieved it, not only in his physiology but also in his treatises on *Electricity* (1827), *Galvanism* (1829) and *Electromagnetism* (1831) which he wrote for the Society for the Diffusion of Useful Knowledge. In 1862, the publisher of the Society's magazine wrote that

> "Amongst the founders of the Society, Dr. Roget was, from his accepted high reputation, the most eminent of its men of science. He wrote its treatises on Electricity and Magnetism .... Upon all questions of physiology, Peter Mark Roget and Charles Bell are the great authorities in the Useful Knowledge Society".[11]

Roget also did his own experimental work: in 1818 he published an account of his investigation into the Kaleidoscope, an optical device that David Brewster (1781-1868), the Scottish physicist and biographer of Newton, had invented recently and patented. Roget read Brewster's account of it, conducted his own investigation, analysed the phenomenon mathematically and in his own paper[12] developed the kaleidoscope principle beyond what Brewster had written. In 1820, he published a paper 'On the voluntary action of the iris',[13] describing how he was able to dilate and contract the iris at will, a surprising technique since these were processes "which are usually considered as no more under the dominion of the will, than the heart or blood-vessels". He concluded that the aperture of the iris is not wholly dependent on the influence of light. Nowadays this might seem a simple topic for investigation, scarcely meriting the name 'experimentation', but we must recognize that until physiologists had discovered which bodily responses were voluntary and which involuntary, the fields of sense-perception and neuro-muscular functions could not be studied properly.[14] Several aspects of these fields could be investigated only by self-experimentation. Roget was not alone in doing such auto-experimentation; two others were Johannes Müller, who published his results, *Ueber die phantastischen Gesichtserscheinungen*, in 1826 (see chap. 13), and Jan Evangelista Purkyně (1787-1869), the Czech physiologist.

One notable quality of Roget's kaleidoscope paper was its mathematical content. Roget was a highly capable mathematician, and it was on the strength of an elegant mathematical paper[15] that he was elected to the Royal Society in 1815. He realized, and emphasized repeatedly in his writings, the rôle of mathematics in any rigorous science, be it physiology or physics. Rigour was consequently stamped on all his work, and his ideas, on power for instance, were therefore more than mere speculation. In this, he differed greatly from a man like Hans Christian Oersted (1777-1851), whose seminal contribution to the study of power was informed greatly by his metaphysical position; Roget knew and reviewed Oersted's work and regretted deeply its metaphysical foundation.[16]

In 1822, he began another series of lectures on physiology and comparative anatomy at the Royal Institution; again, he was a great success and it was no surprise that when the Fullerian chair in physiology was created in 1834, Roget became its first occupant, retaining it for three years. In the 1820s he was also engaged in two public health investigations, one on a fever epidemic which had erupted at the new Bentham-designed Millbank Penitentiary,[17] and the other on London's water supply.[18] Like most reports submitted to a government dedicated to *laissez-faire* philosophy, they were largely unfruitful, but one result of the second report was that sand-filtration systems were built and became the standard method of water purification in England.

In December 1824 Roget read a paper to the Royal Society on the strange phenomenon he had noticed when looking at a moving carriage-wheel through venetian blinds in a window of his home.[19] The slats of the blind broke the movement of the wheel into a succession of still pictures. Moreover, the spokes of the wheel then appeared curved, and Roget found that such curvature depended on the velocity of the carriage and the observer's position relative to the blind; he proposed that the phenomenon depended on the persistence of each still picture on the retina for a small but significant period after the light rays have ceased arriving. This paper was one of the foundation stones of the motion-picture film, and a century later there were articles in *The Photographic Journal, The Illustrated London News, The Times* and other journals commemorating Roget's work.[20]

In the 1820s Roget was busy promoting the Useful Knowledge Society, of which he was a founder member. His essays on electricity and magnetism, mentioned above, were thorough and learned discussions on power or force. In his *Bridgewater treatise on physiology* (1834) he was also interested in the question of power. Indeed, Roget is an excellent example of a physiologist whose dynamical approach to physiology was thoroughly informed by his knowledge in the physical sciences, and whose physiology was read by, and presumably influenced, many of his contemporaries. The *Bridgewater treatise,* as the *Dictionary of National Biography* emphasizes, went through four editions during his lifetime.

To conclude this account of his career: after years of wrangling with several members of the Royal Society, especially Charles Babbage (1792-1871), Sir Charles Lyell (1797-1875), Leonard Horner (1785-1864) and Sir John Herschel (1792-1871), he resigned his secretaryship in 1848 and devoted his time to preparing his *Thesaurus* for publication. The first edition appeared in 1852,[21] and by his death in 1869 he had seen it through 25 editions!

The *Thesaurus* gives us a few clues to the character of Roget's thought in general and his scientific work in particular. The first point to realize is that, although it is considered today to be a book of synonyms, Roget never envisaged it as such; he denied that two words could ever have the same meaning and he preferred to use the expressions 'analogous' or 'correlative' for similar words. The *Thesaurus* was nothing so prosaic as a mere list of words; rather, it was a list of ideas, and Roget considered it to be his philosophic *magnum opus* as well as his philanthropic contribution to mankind. This was explained in his preface.

> "The use of language is not confined to its being the medium through which we communicate our ideas to one another; it fulfills a no less important function as an instrument of thought; not being merely its vehicle, but giving it wings for flight. Metaphysicians are agreed that scarcely any of our intellectual operations could be carried on to any considerable extent without the agency of words .... Into every process of reasoning, language enters as an essential element. Words are the instruments by which we form our abstractions, by which we fashion and embody our ideas .... It is on this ground, also, that the present work founds a claim to utility. The review of a catalogue of words of analogous signification will often suggest by association other trains of thought which, presenting the subject under new and varied aspects, will vastly expand the view of our mental vision".[22]

Perhaps it is mere coincidence that this passage sounds remarkably similar to Coleridge's poetry criticism. (Coleridge, let us remember, had been a regular visitor to

Beddoes' Pneumatic Institute and he, Humphrey Davy and Roget were there contemporaneously. In later years, all three were in London together, and though Roget never mentioned Coleridge or other Romantics in his scientific writings or his *Thesaurus*, it might well be fruitful to enquire into his contacts with, and impressions of, Coleridge and others. To my knowledge, this has never been done; even Emblen makes only passing references to Coleridge). Clearly, like Coleridge, Roget believed that the correct usage of words could be invaluable to "expand the sphere of our mental vision" and obtain an overall view of things. Such was the aim of his private notebook of ideas and their words which he began to compile in 1805 and which he expounded continuously until 1852 when it saw the light of day as the *Thesaurus*. Such, too, was the aim of his scientific work, for by classifying and relating raw observations one to another he hoped to give a connected, comprehensive view of his subject. As for the philanthropic aim of the *Thesaurus* Roget hoped that his system would be used for other languages and that a universal philosophical language would emerge. He followed the traditional belief that one of the greatest barriers to the interchange of thought and mutual understanding amongst men was the diversity of human language.[23] In this, he realized well the power of words and warned against their careless usage. When, therefore, we read his scientific writings we may be confident that he meant what he wrote and was not prone to take refuge behind ambiguous analogies and magnificient metaphors. He himself warned against this all-too-prevalent tendency of his day in his critique of phrenology, entitled

'Cranioscopy',[24] written for the *E.B. Supplement*. There, he argued that the only basis for phrenology was a series of analogical reasonings; but since analogies could only serve to direct and stimulate further inquiries, and could never prove identities, it was simply "a gross violation of logic" to construct a whole new science thereon. In his considered opinion, whaterever the practical efficacy of phrenology might be, its theoretical infrastructure "would have been so capable of affording Swift a new incident for the history of the philosophers of Laputa".

Bearing in mind Roget's belief in accurate and judicious use of words and logical argument, let us examine his ideas on power. Power loomed large in the following works: from his physiology, his *Introductory lecture on human and comparative phusiology* (1826), his *Bridgewater treatise* (1834) and his two essays on physiology and phrenology for the 7th edition of the *E.B.*, which were published together as a book in 1838; from his physical science, there were his essays for the Useful Knowledge Society, where he reviewed brilliantly and gave his own opinions on *Electricity, Galvanism, Magnetism* and *Electro-magnetism*. These essays were so meticulous and incisive that one cannot help thinking they must have ap~~p~~ealled more to professional natural philosophers and mathematicians than to ordinary readers, although the lucidity of his style often made an intricate issue easily comprehensible. I shall also discuss a philosophical *jeu d'esprit* he wrote in 1828, 'On an apparent violation of the law of continuity'.

Roget's dynamical approach to physiology can be seen in two early encyclopaedia articles. In 'Tetanus',[25] which he wrote for *Rees's Cyclopaedia*, he described the last stage of the disease as the decline of the powers of life, due to the vast expenditure of energy by the violent muscular actions; death was due to exhaustion alone. His cure for tetanus depended on a theory, quite popular at that time, called 'counter-irritation',[26] whereby the living body was supposed to possess a certain stock of power, especially nervous power, which was partially consumed with every action of the organism and could not be replenished until sufficient food and rest had been supplied; disease, being a dynamical process, consumed a portion of the vital power; but if the physician could so employ the body's powers that none were left for the disease agent to "feed upon", the agent's activity would be suspended and the disease would die away. Behind this theory was the tacit assumption that vital power was not *sui generis* but was correlated somehow with other forms of power, chemical powers for instance, from which it could be made. In Roget's cure Cullen's ideas on the primacy of nervous energy were evident.

> "The plan from which theory would lead us to expect most success is that of exciting some new action in these organs /brain and nervous system/, by which their energies would be directed into some different channel, and the exciting, morbid action would be suspended and superceded. The remedies which exert the most powerful immediate effects in the nervous system are accordingly found to be the most efficacious in the cure of tetanus: Opium, wine and other highly diffusible stimuli,

digitalis and other narcotics, the sudden affusion of cold water ...."[27]

In his article on 'Age'[28] for the *Cyclopaedia of practical medicine* (1833), Roget was concerned with the interdependence of the vital powers, (the old idea of sympathies), and the preservation of the balance and total quantity of those powers; he used an analogy between a watch whose mainspring is running down, (a mechanism in which power was admitted to be neither created nor destroyed), and the ageing vital system. Death was due to exhaustion of energy, the result of the consumption of power exceeding its supply. He supported the hypothesis that a certain stock of vital power is given to the embryo at its formation, as a provision for life;"that in every action, a portion of this power is expended, 'till at length the whole is consumed". This was a mechanical notion; yet he recognized a distinction between organic and inorganic systems - the latter owed their destruction to external causes, whereas living systems were consumed from within, "being consumed by the very fire which is itself the source of their animation". This distinction did not imply that organisms could generate their own power, for bodies that are "consumed by the very fire which is itself the source of their animation" is also a description of a flame - to wit, Mr. Christie's explanation of the Brunonian system. And no-one considered a flame to generate its own power.

In his *Introductory lecture on human and comparative physiology* Roget explained both his dynamical approach and his position on vitalism quite unequivocally:

"The phenomena of life result from the conjoined and harmonized operation of those several powers /ma/nifest in life itself/; and it is one of the great objects of the science of physiology to ascertain the laws of those physical powers .... This has hitherto received less attention than its importance demands: and physiologists have in all ages shown too great an eagerness to attempt a reduction of all the phenomena to a single principle, or law of life ...."[29]

Physical powers were thus admitted as connected closely with life itself. This did not prevent him from using the terminology of a distinctive vital force, for he believed vital force to be a final, not an efficient, cause. Like many others, he was wonderfully impressed with Paley's *Natural theology* where the teleology of the world, and especially of life, was spelt out. Although acknowledging the reality of final causes, however, Roget emphasized that they belonged to metaphysics and not to physiology, at least, not to his new science of physiology.[30] Vital force, though real, belonged not to the province of the physiologist; only physical forces belonged there. For that reason, he differed from his eminent colleague, Sir Charles Bell, in his evaluation of recent physiological developments. Take John Hunter's work for instance. Bell had praised it, especially for Hunter's theory of the blood. Hunter had said that blood

"coagulates from an impression: that is, its fluidity under such circumstances being improper or no longer necessary, it coagulates now to answer the necessary purpose of solidity. ... for I have reason to believe that blood

> has power of action within itself,
> according to the stimulus of necessity;
> which necessity arises out of its situation".[31]

Roget rejected such usurpation of physically efficient powers by a final cause, even though he acknowledged John Hunter to be "the greatest physiologist since the days of Haller". In his opinion, a similar usurpation had led to the doctrines of the *archeus* and *anima* of Van Helmont and Stahl; and the *vis medicatrix naturae*, which Hoffmann and Cullen considered so important, had the same origin.[32]

The temptation to find a primary source of power with which all other types could be correlated, and which would set agoing and maintain a system, be it inorganic or organic, revealed itself in Roget's discussion of light. It reminds one of the almost magical importance of light in the growth of European science since the time of Robert Grosseteste (c.1175-1253) and Roger Bacon (1214-1292),[33] an importance that it had still for James Hutton and was yet to have for John Draper's world view. Indeed, light has been one of the most alluring, divine and poetic issues that European natural philosophers have ever studied, and it is not surprising that Roget was in rare metaphysical mood when he discussed it:

> "We first avail ourselves of the power
> of light; that divine and almost spiritual
> essence, which seems especially formed for
> the use and enjoyment of intelligent and
> sentient beings".[34]

This primacy of light led him to ask after the primary power within life; human machinery would employ steam, he wrote, but none of the inorganic forces could be admitted conveniently as the primary power within life. So the organic analogue to steam-power was muscular-power.[35]

The quest for primary powers was reopened in his *Bridgewater treatise*.[36] There, he sought as mechanistic an account of life as was compatible with the overall reign of vital force as a final cause. He sought, above all, that transcendent power by which mere matter became animated by the breath of life and became capable of activity, sensation, perception and intelligence. That exalted first power was, of course, God, and from physiology Roget was sanguine enough to expect a clue. Indeed, he believed that sufficient clues had been found to indicate that the fundamental manifestation of God's creative power in life was muscular contractility, and in discussing it he analysed the similarity and distinction between organic and inorganic systems; clearly, the latter could never create power.

> "It is an established principle in physics that mere machinery is incapable of generating mechanical force; and that such force must always originally be derived from some extraneous source. Some impulse from without, whether it be the pressure of the wind, the fall of a stream of water, or the action of men or horses, or any other kind of foreign agency, must be resorted to, both to set the engine in motion and to continue its movements when they are once begun. Nor is the case essentially different when the source of motion apparently resides in some internal part of the machine itself; in a watch, for instance, which is actuated by the main spring ...."[37]

But the living body was different, for its internal principle of motion, "as far as we can perceive", could not be referred to any of the primary inorganic forces. Hence, muscular contractility seemed to be the *primum movens* of life. Elsewhere, however, he entertained respiration as a source of power for living systems even more fundamental than contractility, thus suggesting a close connexion between inorganic, chemical power and organic power. He seems to have been onto the same idea as Marshall Hall,[38] though Hall was not cited.

> "Combined with the particular mode of circulation /of the blood/, it /respiration/ affords a tolerably accurate criterion of the energy of the vital powers. In birds, the muscular activity is raised to the highest degree, in consequence of the double effect of the air upon the whole circulating blood in the respiratory organs. The mammalia rank next below birds in the scale of vital energy; but they still possess a double circulation and breathe atmospheric air. The torpid and cold-blooded reptiles are separated from mammalia by a very wide interval, because although they respire air, that air only influences a part of the blood ...."[39]

In the case of animal heat, Roget admitted that the connexion with respiration, and hence with chemical energy, was a causal one.[40] In the case of other vital activities and powers he was more cautious, but the possibility was left open: for instance, the perfection of plant sap could not occur unless exposed to the chemical agency of air in the leaves, and blood seemed to require the perpetual renovation of its vitality by the influence of respiration.

In direct disagreement with Charles Bell and John Hunter, he asserted that "even the circulation of these juices is an object of inferior importance compared with their aeration".[41] Though he did not admit it explicitly, Roget seemed sometimes to believe in respiration as the power by which the spring of the living clock is kept wound up. This very metaphor was to be championed by Justus Liebig a few years later.

The paramounce of power was evident especially in the closing paragraphs of the *Bridgewater treatise*; Roget reviewed the great enigmas in the world yet to be resolved, among which was the nature of dynamic agents:

> "... how narrow is the field of our perceptions, and how far distant from any approximation to a knowledge of the essence of matter, of the source of its powers, or even of the ultimate configuration of its parts! How remote from all human cognizance are the intimate properties of these imponderable agents, light, heat and electricity, which pervade space and exercise so potent a control over all the bodies in nature".[42]

In his *Treatise on physiology* (1838),[43] which appeared first as an essay in the 7th edition of the *E.B.*, Roget developed some of his earlier ideas, in particular the nature of vitality and the interrelations of powers. Still emphasizing the uniqueness of living systems in their obedience to final causes, he went far in suggesting that they shared a common set of efficient causes. Vital affinities, he suggested, might be none other than ordinary chemical affinities operating under peculiar conditions, and every fresh discovery in animal and vegetable chemistry would help to remove the apparent differences. His was a remarkably clear account of what T.S. Hall[44] and Owsei Temkin[45] call 'vital materialism'. All this was part of his belief in the ultimate simplicity of Nature, but Roget was too cautious not to recognize that such a belief was a metaphysical point of view and not a proven principle of Nature. Therefore,

> "It is possible, or even probable, that future researches may be successful in establishing the identity of some of the powers we now conceive to be distinct, with other powers already known. Thus in the physical sciences the recent discoveries which have taken place in electro-magnetism have satisfactorily established the identity of the magnetic and electric agencies. The same may possibly be accomplished in future times with respect to heat and light .... But no such approximation can yet be attempted with any prospect of success between the muscular, thesensorial, the nervous and the organic powers. No speculative ingenuity can reduce them to a single physical power ...."[46]

Despite his avoidance of 'speculative ingenuity', Roget's belief in Nature's simplicity led him to discuss other reductionist hypotheses in physiology. For instance, there had been attempts to find the fundamental building block of living systems, of which the most plausible seemed to be the recent granular or globular one which, as Roget asserted, was most evident in the composition of chyle, blood and the secretions. Although he was not aware that such globules were usually artefacts of microscopic techniques,[47] he did admit that though such studies had great prospect of success, they still suffered from inadequate experimental tools in the medical sciences.

Experimental techniques in the physical sciences, however, had become so rigorous that he believed new dynamical principles had been established recently. These principles unfolded themselves in his four articles for the Useful Knowledge Society between 1827 and 1831, in the last of which he discussed them most explicitly. In the first article, 'Electricity', he recorded the immense debt of natural philosophy to Humphrey Davy:

> "... it was not until the present century that the extensive relations which connect electricity with so many other branches of physical science were discovered, and their importance appreciated ... hence have we been able to trace alliances between several of the great agents concerned in the phenomena of the material universe. Electrochemistry has thus arisen as one of the connecting branches between the Philosophy of Nature. Still more recently there has been opened to us, in the subject of Electro-Magnetism, another new province of science, which establishes a natural connexion between two powers hitherto regarded as distinct".[48]

He discussed experimental evidence for the evolution of heat from electricity, of chemical forces from electricity, and of mechanical power from electricity, but he nowhere philosophized upon any fundamental connexions among such powers.

More definite ideas on power connexions came in the article 'Galvanism',(1829). In Davy's chemical theory of galvanism he saw a rigorous causal connexion between chemical and electrical forces: "the energy of the galvanic power will depend altogether upon that of the chemical action, and can never be excited when the latter condition is wanting".[49] The concluding paragraphs of the article discussed Volta's contact hypothesis for galvanism, Roget's rejection of which was based upon his belief in 1) A correlation or inter-convertibility of the inorganic powers; and 2) A conservation of power in all such conversions. On the correlation issue, the association between chemical action and electrical effects occurred so invariably that all just rules of philosophy would admit a causal relation; indeed, the quantity of galvanic effect seemed always to be proportional to the energy of the chemical action. In addition, he had a more general argument:

> "If there could exist a power having the property ascribed to it by the /Voltaic Contact/ hypothesis, namely, that of giving continued impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other powers in Nature. All the powers and sources of motion, with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect; or in other words a perpetual motion".[50]

Such, he said, were the consequences of Volta's hypothesis, and against the validity of such consequences the probability was all but infinite.

The article on 'Magnetism' (1831) did not say anything new about forces, though it was concerned largely with their connexions. Perhaps the only suggestion of the direction of his thoughts was the declaration that no great or comprehensive fact in science was ever established without being preceded by a bold conjecture, and that hypothesis is invariably the precursor of truth.[51]

Coming now to the most important article in the series, 'Electro-Magnetism' (1831), Roget first outlined the history of the subject. There had been much speculation about a correlation between electricity and magnetism in the 18th century, and he cited a prize essay question that the Electoral Academy of Bavaria had set in 1774:

> "Is there a real and physical analogy
> between electric and magnetic forces;
> and if such analogy exist, in what manner
> do these forces act upon the animal body?"[52]

Some of the essays had supported the analogy, and Roget maintained that recent discoveries had gone further by showing that they emanate from a common source. Moreover, bearing in mind the ideas mooted in his essay on 'Galvanism', he probably envisaged a correlation among all inorganic powers.

The essay question of the Bavarian Academy is also interesting as it exemplifies the use that medical theory tries to make of discoveries in physical science. There is a dearth of secondary literature on the use of electricity in therapeutics from the Hippocratic Corpus onwards.[53] Its great popularity in European medicine began with the discovery of the Leyden Jar in 1745, for that permitted the delivery of far greater shocks than the older static machines. Between 1750 and 1780, according to one medical historian,[54] no less than twenty-six papers on medical uses of electricity appeared in the *Journal de médecine* alone. An electric shock machine was installed in the Middlesex Hospital in London in 1767, and within the next decade many other English hospitals followed suit. In turn, the similarity between the shock of a Leyden Jar and that obtained from electric fishes led physicians back to using such fish as *Malopterurus electricus* and *Gymnotus electricus* for therapy.

Returning to Roget's paper, after the historical introduction he discussed the work of Hans Christian Oested, and, though praising him for his incontrovertible experimental evidence on the correlation of electrical and magnetic powers, he regretted that his theoretic analysis was too speculative to be intelligible.[55] Whereas Oested was a *Naturphilosophe*, Roget saw himself as a cautious reviewer and experimentalist; his article was therefore a highly technical one, with much experimental detail and mathematical analysis, especially of André Marie Ampère's (1775-1836) electro-dynamic theory, whose rigour and elegance much attracted him. To test Ampére's theory, Roget did an experiment with Michael Faraday in the Royal Institution. Soon after hearing of Ampère's discovery that two parallel wires carrying currents of opposite directions attracted each other, Roget wondered whether such an attraction could be observed between successive turns of a heliacal wire coil if the wire was sufficiently fine. He and Faraday wound a fine harpsichord wire into a helix; on passing a voltaic current through it, it immediately contracted, recovering its original dimensions as soon as the current was stopped. Roget's conclusion is interesting:

> "It was supposed that possibly some analogy might hereafter be found to exist between the phenomenon and the contraction of muscular fibres ...."[56]

Jean Louis Prévost (1790-1850) and Jean Baptiste Dumas (1800-1884) had advanced a similar theory of muscle contraction, but in Roget's cautious opinion it was still too hypothetical an explanation to merit serious consideration. Such admirable and scientific caution, as Emblen suggests, is probably why Roget never hit the headlines so far as historians of science are concerned. Despite his caution, however, Roget hinted repeatedly at fundamental connexions among electricity, magnetism, electromagnetism and neuro-muscular activity. These hints were supported by Ampère's theory, whose mathematical construction Roget understood fully; indeed, his understanding and admiration of the theoretical, mathematical labours of the French philosophers made him an unusual figure in British science, and in this respect he had a decided advantage over many of his colleagues; even physical scientists like Faraday rarely possessed the mathematical skill of Roget. Roget's support of Ampère was also due to the simplicity and beauty of his theory;[57] the lack of such simple elegance was the main reason for his rejecting a rival hypothesis of Wollaston and Oested, for Newton's dictum *Natura simplex est* was as powerful an arbiter as it had ever been.

Roget's essay next discussed experiments and hypothesis on thermo-electricity (the Seebeck effect), the influence of light on magnetism, terrestrial magnetism and Ampère's field. Finally, be announced a grand idea which seems to have been intended as the climax of all four essays; in it, we cannot but recognize the influence of Davy:

> "On the whole, then, it must be allowed that there are strong grounds for the belief that there subsists some mutual connexion, or rather an intimate relation and affinity, between the several imponderable agents, namely Heat, Light, Electricity and Magnetism, which pervade in so mysterious a manner all the realms of space, and which exert so powerful an influence over all the phenomena of the universe".[58]

Roget clearly believed this principle to lie at the very root of natural philosophy; indeed, as we have seen, he was to conclude his *Bridgewater treatise* three years later on exactly the same theme. Admittedly, that theme was not original with him, as his own historical introduction showed. Where he did make his own contribution was in trying to gather adequate experimental and mathematical evidence so that the correlation of Nature's powers would no longer be only a speculation. His was a most thorough discussion of the subject, as we can appreciate by comparing it with Mary Somerville's (1780-1872) book *On the connexion of the physical sciences* (1834).[59] Mrs. Somerville was a fine mathematician and physicist, and although her book was intended to be a popular presentation of physical science, it was thoroughly up-to-date and was very highly regarded by everyone, laymen and scholars alike. (Perhaps the only significant critic was Miss Marian Evans, alias George Eliot, though her criticism was not based on any scientific evidence). Roget's four essays, especially the last, were easily as critical, well-informed and discursive as Mrs. Somerville's book, and it is strange that although they were praised unreservedly by his contemporaries, historians of the physical sciences have rarely even mentioned them.

Finally, let us examine Roget's philosophical article, 'On an apparent violation of the Law of Continuity',[60] first published in 1825. It was essentially a philosophical teazer, written more for light intellectual exercise than as a solemn problem. It concerns us for two reasons - it displays his interest in mathematics, and it was a rare occasion when he discussed explicitly features of the philosophical background of his, and his contemporaries' science. The two philosophers he discussed most were Leibniz and (who else but?) Boscovich. In Roget's estimation, Leibniz's greatest legacy was his law of continuity; *natura non operatur per saltum* was his law of the universe. Boscovich assumed this law as the foundation of his "ingenious and profound theory of natural philosophy, and deduced from it a variety of important corollaries and conclusions".[61] One of those conclusions that Roget discussed was that the continuity law appeared to apply to all physical changes of situation, quality or anything else which are connected in mathematically expressible relations, and that even the forces and powers of Nature vary by continuous gradations from one period to another, without suffering any abrupt transition.[62] This raises the question, for us, of how such changes could be continuous if force or power could ever be created or destroyed. Roget did not ask this question explicitly and did not suggest that Boscovich had; but he did write that

> "... all the changes of magnitude in those quantities of which the value is dependent on that of certain other quantities, accompany corresponding changes in these latter quantities, in a manner strictly conformable with the law of continuity".[63]

Among these quantities he had the powers of Nature in mind, for two pages earlier he had discussed such powers vis-à-vis the continuity principle. One import of the above last passage was, therefore, that in the connexions among Nature's powers, no change in intensity could occur in any one power without a corresponding change in another, since Roget was coming to believe, as his four essays on electricity and magnetism were to show, that power or force is interdependent in all its forms. Thus, in general terms, *nihil ex nihilo*. Furthermore, although he did not specify it, it would have been a breach of continuity if any power were to be destructible, and we may surmise that Roget would have admitted this even for vital economies. How else should an historian interpret Roget's whole-hearted approval of Boscovich's conclusion "... that the law of continuity is essentially universal, and that a breach of it is *metaphysically* [my italics_7_, as well as physically, impossible?"[64]

The continuity law denied the possibility of creation or destruction of any thing or agent in Nature. In the case of one agent producing another, as in voltaic electricity, it indicated to writers like Roget that a true correlation existed between them. Roget was not alone in accepting the continuity law; most of his contemporaries did too, although few wrote papers on it unless they were pure philosophers. Roget's paper, presented a purely mathematical challenge, which he fervently hoped the continuity law would solve with the utmost rigour since he had already decided that it had "a necessary existence in the nature of things".[65] Inevitably, he found the solution and continuity remained secure.

Summary

Power or force, which Roget used synonymously, played a central rôle in his natural philosophy and physiology. One cannot but be impressed by how scrupulously he tried to give evidential and mathematical rigour to that and other general ideas, and one has to admit the depth of his understanding and praiseworthiness of his caution. A reading of all his major works and many of his minor ones has convinced me that, amongst the ideas that he grappled with and eventually espoused, were the correlation, and almost by implication, the conservation of powers. In the former he had a clear belief by 1831 at the latest when he wrote the essay on electromagnetism; and although he did no quantitatively conclusive experiments on it, it was an utterly sober belief and it permeated all his work. As for his failure to enunciate explicitly the principle of power conservation, that is scarcely surprising; the conceptual world within which he had been reared was that of the late 18th century and he consequently had many debates and conceptual difficulties to plough through before he could express himself clearly and confidently on the power question. Just one example of such difficulties was his need to decide on the relative merits and proper provinces of efficient and final causes; as a physiologist, the validity of final causes was obvious (as it is even in the 20th century, to wit H. Driesch,[66] L. Richmond Wheeler[67] and J.S. Haldane[68]); yet as a physical philosopher final causes were taboo. We must remember that his formative years were closer to the spirit of Boscovich, Hutton and John Brown than to that of the experimental and totally mechanistic 'scientist'. By the 1840s, when other men were beginning to enunciate boldly the ideas of the correlation of forces and the conservation of power, Roget was already an old man; by then his driving interest was becoming his *Thesaurus* and besides, as Emblen points out,[69] he was becoming embittered by the squabbling within the Royal Society, and we may be sure that the life of a busy London natural philosopher no longer enchanted him. From the early 1840s he contributed less and less to physiology and physical science and in 1848 he retired, a bitter man, from the senior secretaryship of the Society. It is therefore not surprising that, to my knowledge, he never commented publicly on the energy studies that were done in the 1840s and 1850s. Perhaps a search through his unpublished manuscript material would reveal any private comments.

Finally, I do not claim a primary place for Roget in the emergence of the correlation and conservation principles, certainly not on the same level as James Prescott Joule (1818-1889) or Helmholtz for instance. But he did contribute to the new, sober study of force or power, and this study was important for him. Roget was much more than the author of a word-classification, and although not a first-rank investigative scientist, he was a central, highly regarded figure in British science for several decades and his writings were read by a large proportion of the scientific community. If his ideas in physiology and on electricity, magnetism and allied topics were not startingly original, neither were they banal and, at least in physiology, he was cited often throughout the 1830s, 1840s and 1850s.

NOTES TO CHAPTER 6.

1. D.L. Emblen, *Peter Mark Roget, the word and the man*, London, Longman, 1970.

2. A.G. Debus (editor), *World who's who in science*, Chicago, A.N. Marquis Co., 1968, p.1439.

3. S. Lee (editor), *Dictionary of national biography*, vol.XVII, London, Smith Elder & Co., 1909, pp.149-151.

4. T.H. Ward, *Men of the reign*, London, George Routledge and sons, 1885, p.767.

5. Anon., *List of the graduates in medicine in the university of Edinburgh from MDCCV to MDCCCLXVI*, Edinburgh, Neill & Co., 1867, p.28. "*Petrus Marcus Roget, Anglus. De Chemicae Affinitatis legibus.*"

6. *Op.cit.*, Note 1 above, p.96.

7. P.M. Roget, 'Cranioscopy', in *Supplement to the fourth, fifth and sixth editions of the Encyclopaedia Britannica*, vol. 1, 1824, pp.419-437.
Also P.M. Roget, 'Phrenology' in *Encyclopaedia Britannica*, 7th edition, 1837, 17:454-473.
And P.M. Roget, *Treatises on physiology and phrenology*, 2 vols., Edinburgh, A. & C. Black, 1838.

8. *Op.cit.*, Note 1 above, pp.132-152 especially.

9. *Supplement*, *op.cit.*, Note 7 above, vol. 1, 1816, p.xx. Contributors were identified by a letter code. Roget was W. In the 7th edition he was Y.

10. *Op.cit.*, Note 1 above, p.135.

11. Charles Knight, *Passages of a working life*, London, Bradbury and Evans, 1864, vol. 2, p.123.

12. P.M. Roget, 'On the kaleidoscope', *Ann.Phil.*, 11:375-378.

13. P.M. Roget, 'On the voluntary action of the iris', in B. Travers (editor), *A synopsis of the diseases of the eye and their treatment*, London, Longman, 1820, pp.71-75.

14. The need for careful auto-experimentation was emphasized by various British and Continental workers of that time. A fairly useful compilation of studies of sense perceptions is N. Pastore, *Selective history of theories of visual perception, 1650-1950*, London, O.U.P., 1971.

15. P.M. Roget, 'Description of a new instrument for performing mechanically the involution and evolution of numbers', *Phil.Trans.*, 1815, 105:9-28. This paper was read before the society for him by William Wollaston on November 7th, 1814.

16. P.M. Roget, 'Electro-magnetism', p.4, in *Treatises on electricity, galvanism, magnetism and electro-magnetism*, London, Society for Diffusion of Useful Knowledge, 1832. (Library of Useful Knowledge, vol.2, *Natural philosophy*).
This was the first collected edition of these four essays for the society. To this edition all my references will refer.

17. 'Report of Drs. Latham and Roget, upon the penitentiary, 4th July 1823', in *Report from the select committee on the state of the penitentiary at Milbank*, App.E, item 12 (July 8). Ordered printed by the House of Commons, 1822. Cited by Emblen, *op.cit.*, Note 1 above, pp.162-170.

18. P.M. Roget, T. Telford & W.T. Brande, *Supply of water in the metropolis*, April 21, 1828. Ordered printed by the House of Commons. Cited by Emblen, *ibid.*, pp.212-219.

19. P.M. Roget, 'Explanation of an optical deception in the appearance of the spokes of a wheel seen through vertical apertures', *Phil.Trans.*, 1825, 115:131-140.

20. Cited by Emblen, *op.cit.*, Note 1 above, p.184.

21. P.M. Roget, *Thesaurus of English words and phrases, classified and arranged so as to facilitate the expression of ideas and assist in literary composition*, London, Longman, Brown, Green and Longman, 1852.

22. *Ibid.*, quoted in a later edition: S.R. Roget (editor), *Thesaurus*, London, Longmans, Green & Co., 1942, p.xv.

23. Cited in *ibid.*, p.xxvii.

24. *Op.cit.*, Note 7 above, p.433.

25. P.M. Roget, 'Tetanus', in *Rees's Cyclopaedia*, vol.35, London, Longman, Hurst, Rees, Orme & Brown, 1819. No pagination.

26. One of the clearest contemporary accounts of counter-irritation theory is in A.B. Granville, *Counter-irritation, its principles and practice*, London, J. Churchill, 1838.

27. *Op.cit.*, Note 25 above, 5th page of the 'Tetanus' article.

28. P.M. Roget, 'Age', in J. Forbes, A. Tweedie and J. Conolly (editors), *Cyclopaedia of practical medicine*, vol. 1, London, Sherwood, Gilbert, and Piper, and Baldwin and Cradock, 1833, pp.34-46.

29. P.M. Roget, *An introductory lecture on human and comparative physiology, delivered at the New Medical School in Aldersgate street*, London, Longmans, Green & Co., 1826, p.18.

30. *Ibid.*, p.22.

31. J. Hunter, *A treatise on the blood*, London, G. Nicol, 1794, p.25.

32. *Op.cit.*, Note 29 above, pp.22-24.

33. The rôle of light in mediaeval natural philosophy is discussed excellently in the following:
A.C. Crombie, *Augustine to Galileo*, vols. 1 & 2, London, Peregrine books, 1969.
A.C. Crombie, *Robert Grosseteste and the origins of experimental science*, Oxford, O.U.P., 1953.
There are also useful discussions in the footnotes to A.I. Sabra, *Theories of light from Descartes to Newton*, London, Oldbourne Book Co. Ltd., 1967.

34. *Op.cit.*, Note 29 above, p.43.

35. *Ibid.*, p.48.

36. P.M. Roget, *Animal and vegetable physiology considered with reference to natural theology, being no.5 of the Bridgewater treatises*, 2 vols., London, W. Pickering, 1834.

37. *Ibid.*, pp.127-128.

38. *Op.cit.*, Chapter 5, Note 31 above.

39. *Op.cit.*, Note 36 above, pp.331-332.

40. *Ibid.*, pp.339-340.

41. *Ibid.*, p.290. On this particular issue he cited many fine experiments by Spallanzani.

42. *Ibid.*, p.638.

43. P.M. Roget, *Treatises on physiology and phrenology*, 2 vols., Edinburgh, A. & C. Black, 1838.

44. T.S. Hall, *Ideas on life and matter*, Chicago, University of Chicago Press, 1969, vol. 2, p.221.

45. O. Temkin, 'Materialism in French and German physiology of the early nineteenth century', *Bull.Hist.Med.*, 1946, 20:322-327.

46. *Op.cit.*, Note 43 above, pp.179-180.

47. On the development of the cell theory and the errors in late 18th - early 19th century studies of micro-structure, a most useful and scholarly account occurs in J.R. Baker, 'The cell-theory: a restatement, history and critique', *Q.Jl.microsc.Sci.*, 1948, 89:103-123; 1949, 90:87-108; 1952, 93:157-190.

48. P.M. Roget, 'Electricity', p.1, in *op.cit.*, Note 16 above.

49. P.M. Roget, 'Galvanism', p.23, in *ibid.*

50. *Ibid.*, p.32.

51. P.M. Roget, 'Magnetism', p.32, in *ibid.*

52. P.M. Roget, 'Electro-magnetism', p.97, in *ibid.*

53. Three useful papers are: P. Kellaway 'The part played by electric fish in the early history of bioelectricity and electrotherapy', *Bull.Hist.Med.*, 1946, 20:112-137.
H.E. Hoff, 'The pre-galvanian electrophysiologists', *Ann.Sci.*, 1936, 1:157-172.
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For a lengthier discussion and useful bibliography, see S. Licht, *History of electrotherapy*, New Haven, E. Licht, 1959.

54. Hoff, *ibid.*, p.163.

55. *Op.cit.*, Note 52 above, p.4.

56. *Ibid.*, p.59.

57. *Ibid.*, p.82.

58. *Ibid.*, p.99.

59. M. Somerville, *On the connexion of the physical sciences*, London, J. Murray, 1834.

60. P.M. Roget, 'On an apparent violation of the law of continuity', *Phil.Mag.*, 1828, 3:118-121 and 203-206. Reprinted from the *Scientific Gazette* of 1825.

61. *Ibid.*, p.118.

62. *Ibid.*, p.119.

63. *Ibid.*, p.121.

64. *Ibid.*, p.121.

65. *Ibid.*, p.121.

66. H.A.E. Driesch, *The history and theory of vitalism*, trans. C.K. Ogden, London, Macmillan, 1914.

67. L.R. Wheeler, *Vitalism: its history and validity*, London, Witherby, 1939.

68. J.S. Haldane, *The philosophy of a biologist*, Oxford, O.U.P., 1935.

69. *Op.cit.*, Note 1 above, pp.236-254.

Chapter 7. William Benjamin Carpenter (1813 -1885)

William Carpenter received his early medical training as a physician's apprentice in Bristol. During that period he attended lectures and used the library at the Bristol Mechanics Institute and also attended the Bristol Medical School. In 1834 he went to London to study medicine for a year, became a member of the Royal College of Surgeons and the Apothecaries Company, and went on to Edinburgh where he took his M.D. in 1839.[1] In Edinburgh, whilst still a student, he made a name for himself as a talented contributor to several medical societies, becoming president of the (students') medical society; at that time he began a special interest in physiology. According to his biographer,[2] at the age of 21 he began to plan a philosophical treatise on natural history, for all the treatises of the day seemed to him deficient in the underlying principles of physiology. He believed passionately that physiology ought to be based on a comparative study of animal and vegetable functions, and his first academic paper was on this theme: 'The structure and functions of the organs of respiration in the animal and vegetable kingdoms' (1835).[3] The comparative approach was again evident in two papers of 1837: 'The voluntary and instinctive actions of living beings',[4] in which he sought a common explanation of the irritabilities of vegetables and animals; and 'The unity of function in organized beings',[5] in which he attempted to apply to functions a law that the Russian naturalist, Karl Ernst von Baer (1792-1876), had enunciated for structures.

In 1837 he was appointed lecturer on medical jurisprudence at the Bristol Medical College. There he won a prize for an essay 'On the differences of the laws regulating vital and physical phenomena', in which he asserted that vital laws are merely emergent characteristics of matter in particular arrangements. Like Roget, he was a vital materialist.

By now, Carpenter had determined not to practise as a physician but to devote his life to write and teach physiology and conduct his own research. In 1839 he published a *General and comparative physiology* and in 1842 his *Principles of human physiology*; during the next 30 years, these books became standard texts in medical education in England and went through several editions. Indeed, despite the scant attention they receive from medical historians today, they seemed to have been well regarded and widely read, even outside Britain: for instance, J.W. Draper cited them in his *Human physiology, statical and dynamical*, and von Baer wrote to him to say how much pleasure he had got from reading one of them whilst on holiday by the Caspian Sea.[6] Sir James Paget's evaluation of his *Physiologies* was as follows:

> "I believe that among all the events that have had great influence on the teaching of physiology in our medical schools, none has been more important than the institution of separate courses of physiological lectures. ... among many things proving its necessity, none I think had more influence than the publication of Dr. Carpenter's two principal works in 1839 and 1842. Their influence coincided with those exercised by Dr. Sharpey's teaching and the translation of Müller's

> *Physiologie des Menschen*, and with the constantly increasing interest in physiology which was stirred by the teachings of Owen, Liebig and Goodsir, by Dr. Marshall Hall's works on the reflex functions of the spinal cord, by Kiernan's essay on the minute structure of the liver, and Bowman's on that of the kidney .... I think that no change more important than this has been made in our medical schools during the last half century; and that no-one contributed to it more than Dr. Carpenter. For many years, his books were almost without rival in the London schools; Mayo's *Physiology* soon ceased to be read; the translations of Tiedemann and Blumenbach were disused; the translation of Müller's *Physiology* was too large, and in some parts too difficult for any but the best students".[7]

A slightly less glowing estimation, but still a creditable one, was given by Thomas Henry Huxley (1825-1895).[8] Carpenter, he said, undertook the important rôle of intermediary between the rapidly accumulating mass of knowledge and the student in physiology; sifting and methodically arranging the data, and presenting them in lucid style, he produced a valuable compendium of physiology which, although containing but few original data of his own, was highly original in the way it treated various topics.

It will be the principal argument of this chapter that one of Carpenter's most original contributions to general physiology was his use of power, that the ideas of the correlation and conservation of powers, especially the former, emerged even in the earliest editions of his *Physiologies*, and that after the formal, full enunciation of the conservation of energy in the late 1840s, he was one of the first physiologists to discuss in detail its applicability to the vital system.

Before discussing his dynamical physiology, mention should be made of his other works. Throughout his career he wrote fine, thoughtful papers on physiology and in 1856 brought out a manual on *The Microscope*[9] which became a scientific best seller in Britain and America. Meanwhile he was elected Fullerian Professor of Physiology at the Royal Institution, was made an F.R.S., began to teach physiology full time at University College and also began his own research on fossil invertebrates. In 1856 he became registrar of the University of London, a post he held for the next 23 years. His fossil researches were not brilliant but they were very competent and show that he was not only a compiler of other men's studies. Hence, Sir James Paget admired his study and extension of Marshall Hall's recent theory of reflex action in the spinal cord, which he did at Edinburgh; and his researches on foraminifera led him towards the evolution theory, on which he gave Charles Darwin (1809-1882) his support; Darwin appreciated such support, especially since he held Carpenter in high regard as a physiologist.[10]

Turning now to Carpenter's physiology and his ideas on power, there is a preliminary point to make which provides a key to his entire natural philosophy. In 1828 he attended a lecture at the Bristol Mechanics' Institute by a Mr. Thomas Exley (1775-1855) on his new theory of matter; greatly excited by it, he read Exley's book, *Principles of natural philosophy, or a new theory of physics* (1829),[11] from which he probably got his introduction to Boscovich and to the notion of matter being explicable wholly in terms of force. As D.M. Knight points out,[12] Exley was not an original or particularly influential figure in 19th century science; but he did reflect the considerable interest in dynamical hypotheses of matter which constituted a powerful under-current amongst some of the foremost intellects of his day. He took his cue from Newton and Boscovich, aiming to construct a simpler dynamic theory of matter than either of them; his theory was that of Boscovich, but pruned by Newton's *regulae philosophandi*; the first sphere of repulsion and the last sphere of attraction of Boscovich were retained, but the intermediate, alternating spheres were discarded, since the Newtonian rules did not allow a multiplicity of causes when few would suffice. He voiced the deepest beliefs of a number of his contemporaries, to wit Humphrey Davy and Michael Faraday, in saying that

> "... it is nothing but mere hypothesis, the effect of imagination and a vulgar notion, to judge that there is a minute, solid, impenetrable mass necessary to constitute an atom of matter on which forces act. I am aware that this notion has been admitted by philosophers of the first rank, but we know nothing of such little solids, we have never seen them, nor felt them, nor perceived them by any of the senses".[13]

Definition 24 constituted the core of his theory:

> "Absolute force of an atom is its force at a given distance from the centre, and this is called its mass or quantity of matter".[14]

Nonetheless, he often used expressions like 'material atom' and 'etherial particle' for they were useful labels, even if not real entities. Of course, this gave his theory and others of the same ilk such an unfair advantage over strictly corpuscular matter-theories, for he could still use what appeared to be an atomic model which in many respects did not differ from, and had the usefulness of, true atomic theories; and when criticized for departing from the paradigm of pure force, he could defend himself by asserting that atoms might be entertained heuristically, even though they were essentially only aggregations of force. This might explain why, despite the blatantly deductive and speculative character of Exley's theory, it did appeal to a fair number of intelligent people; it must have had a consistent appeal throughout the next twenty years for he again lectured on it at a meeting of the British Association for the Advancement of Science in 1848.[15] It is not surprising, therefore, that young William Carpenter was excited by it. Nor is it surprising that Carpenter never referred to it in his scientific writings, for he seems to have realized quickly how a-prioristic and speculative it was. Nonetheless, the primacy of power permanently informed his science. We shall now examine this.

Throughout his career in physiology Carpenter sought functional and dynamical relations within the vital economy; unlike his 18th century precursors, he did not use the largely metaphysical vital powers such as *vis nervosa*, *vis insita* and *vis medicatrix*, but relied mostly on recent empirical studies by British and Continental researchers. One instance of this was his idea of the essential unity, or correlation, of vital functions, as discussed in his paper 'On the unity of function in organized beings' (1837). His aim was

> "... to apply to function one of the laws propounded by Von Baer with regard to structure, namely that 1) A special function arises only out of one more general, and this by a gradual change. To this law I shall add a second. 2) In all cases where the different functions are highly specialized, the general structure retains, more or less, the primitive community of function which originally characterized it".[16]

An obvious field in which these laws could be tested was the sense-perceptions, for several physiologists had been investigating them experimentally with promising results. Carpenter speculated that the special functions of sight, hearing, smell and taste might be merely elaborations out of the general sense of touch. Such unity of functions he hoped also to find in the broader field of comparative plant and animal physiology. It agreed too with his belief in the ultimate unity or correlation of the powers behind the inorganic phenomena of Nature; such was evident in his paper 'Physiology, an inductive science' (1838), where he put a Newtonian-like query:

"Is it possible that these physical and vital properties of matter, which are at present our ultimate facts and axioms, may be hereafter included in a more general expression common to both? On this subject we can only speculate; but the probability appears decidedly in the affirmative. We have already remarked upon the rapid progress of generalization in the physical sciences, rendering it probable that before long one simple formula shall comprehend all the phenomena of the inorganic world; and it is not perhaps too much to hope for a corresponding simplification in the laws of the organized creation.... In the proportion to our attainment of such generalizations, we rise from the domain of our ignorance to that of our knowledge ...."[17]

This vision of two all-embracing laws (ultimately, perhaps, just one law) for Nature was clearly well in line with the Newton-Boscovich-Exley tradition. It also had theological value for Carpenter, (as it had, too, for Newton and Boscovich), in that every step towards a comprehensive theory of Nature was further evidence of the beauty and harmony of the world and of the mind of its Creator.[18]

The quest for a general, unifying theory for both vital and inorganic powers can be seen in his *General and comparative physiology* (1839).[19] In its concluding paragraphs much of the argument of his 1838 paper was reproduced, but it went into the question of unity much more intensively. Carpenter's main aim seems to have been a true mean between extreme vitalism and extreme mechanicism, an aim which was facilitated by his thorough knowledge of the latest physico-chemical researches in physiology. An especially interesting approach was his discussion of William Paley's 'Principle of compensation', according to which all living organisms were supposed to be constructed on only a few basic patterns, each pattern being characterized by a certain quantity of material. In Paley's own words,

> "Compensation is a species of relation. It is relation when the defects of one part, or of one organ, are supplied by the structure of another part or of another organ. Thus, 1. The short, unbending neck of the elephant is compensated by the length and flexibility of his proboscis. He could not have reached the ground without it.... To a form, therefore, in some respects necessary, but in some respects also inadequate to the occasion of the animal, a supplement is added, which exactly makes up the deficiency under which he laboured".[20]

It was as if the living economy had only a certain quantity of material available for its construction, and like all limited economies, if one section was unusually well supplied, some other section had to suffer a deficit. With Paley, this principle of compensation had been wholly anatomical, concerning bodily structures rather than functions. Even when used physiologically, the functions were explicated in material terms, such as foodstuffs and secretions, rather than as powers. Carpenter shared this materialist approach, but he also applied the principle to the body's powers, whereby the power supplied by food and air was envisaged as the sole source of vital power, and its excessive employment in one physiological function would be compensated by a corresponding reduced employment in another function. Thus, in plants there was an antagonism between the nutritive and reproductive functions, the one being executed at the expense of the other. A similar antagonism existed in the animal kingdom, with the added complication of a third set of functions, namely the sensory and locomotive. The highest form of such competition for the power that an animal imbibed was the relation between the blood and air, namely the function of respiration.

> "The dependance of the organism on the constant stimulus of the circulating fluid is more evident in proportion as, in ascending the scale, we meet with greater variety and activity in the vital operations. The maintainance of the vivifying powers of this fluid by its exposure to the atmosphere is therefore demanded more urgently than the mere supply of its deficiency by the ingestion of fresh aliment; and it is accordingly found that many animals are capable of subsisting a considerable time without nourishment, whilst there are few which do not speedily perish, or whose *vital actions* at least are not checked, when deprived of air. The correspondence between the activity of this function in any individual system, and its general vital energy, must be evident to the discriminating observer; the comparative energy of the respiration in the active and rapacious eagle, and in the timid and indolent tortoise, afford a ready illustration of the connection. The development of the locomotive powers, and the degree of heat maintained in the system, which may be regarded as pretty constant indications of the general

> activity of its organic functions, will be found peculiarly connected with that of respiration".[21]

Since he viewed respiration as a source of chemical power, the intimate connexion between it and organic activity was tantamount to a connexion between inorganic and organic force or power. Another such connexion concerned animal electricity. Like so many of his contemporaries and late 18th - early 19th century predecessors, Carpenter placed great hope in electricity; because of its ubiquity throughout organic and inorganic Nature, (on which Davy had expatiated so eloquently), and because of its sheer power, (Davy had constructed a gigantic pile of 2000 plates by means of public subscripion), he and his colleagues saw it as the most promising bond between the realms of life and non-life. Historians have discussed in detail this pivotal position of electricity in 18th and 19th century physiology and natural philosophy, and as T.S. Hall, for instance, shows, there were able physiologists who were eager to make electricity the very essence of life - to wit John Abernethy (1764-1831).[22] Carpenter's interest was more restrained than Abernethy's and others', for he was always reluctant to speculate beyond the bounds of empirical evidence; yet there was much evidence for the deep involvement of ordinary, i.e. voltaic, electricity in life. If a prune were sliced in two and the juice squeezed from its halves into separate containers, those containers would acquire opposite electrical states, even though the juice was of uniform chemical composition.[23] Such observations had set physiologists speculating. William Wollaston constructed an elegant electrical theory of secretion,[24] and William Prout (1785-1850) suggested that the small quantities of minerals in vegetable tissues, usually regarded as accidental, might play an essential rôle in a plant's electrical composition.[25] As for the sheer power of electricity, Sir John Herschel (1792-1871) had shown that a force 50,000 times that of gravity might be generated instantaneously by the galvanic action of mercury amalgam on a millionth part of its own weight of sodium. Electricity was even implicated in human behaviour; in men, it was mostly positive, and irritable men of sanguine temperament had more free electricity than those of phlegmatic temperament. Careful studies had shown that the circumstances most conducive to the generation of electricity by the human organism were a temperature of c. 80°F., tranquility of mind and social enjoyment, whilst a low temperature and depressed emotions diminished it correspondingly.[26]

Although Carpenter's discussion of functions and energies in this first edition of his treatise did not culminate in an assertion of some grand principle of power, it is reasonable to suggest that he had one in mind for he admitted as much in the treatise's penultimate paragraph:

> "It has been one object of the foregoing pages to show that *vital* properties are as essentially connected with certain forms of matter, as are those usually denominated *physical* with matter under its more common aspects. One more question yet remains. Is it possible that the physical and vital properties of matter, which are at present our ultimate facts and axioms, may be included within a more general expression,

common to both?"[27]

This general expression, as later editions of his *Physiologies* revealed, was none other than the correlation of all forms of power and the increatibility and indestructibility of power itself. Taking his *Physiologies* in chronological order, we find nothing new in the second edition of *General and comparative physiology* (1841).[28] In 1842 appeared the first edition of his *Principles of human physiology*,[29] where he aimed to collate the valuable results scattered throughout numerous specialized monographs that had been published recently in physiology and medicine; clearly, he had read many up-to-date papers, including some fine continental ones. Power was again a central concept, even more obvious than in the 1839 treatise. This was especially evident in the sections on respiration and sexual activity. On respiration, he discussed the rigorous experiments on bees by a Mr. George Newport (1803-1854) who had shown that the quantity of oxygen they consumed was exactly proportional to the heat they evolved.[30] Carpenter was aware of a similar conclusion that had been drawn from experiments on higher animals: needles had been inserted into active muscles and connected to a thermo-multiplier. Hence he proposed that throughout the animal economy, the development of heat is strictly proportional to the activities of the muscular processes which constitute the nutritive, secretive and other functions. It was then easy to explain the influence of the nervous system on animal heat production.[31] Carpenter believed that animal heat was totally due to the chemical power extracted from respiration and tissue activity, and that muscle activity was intimately connected with respiration since the latter was a tissue-based process and was not, as Lavoisier had said, localized in the lungs. The idea of respiration as a tissue-based process was gaining ground already but, as Everett Mendelsohn,[32] June Goodfield[33] and others have shown, it was still a debatable issue. Carpenter was well up on that debate. Once again, therefore, Carpenter asserted that a power that had been attributed to vitality, namely animal heat, was actually derived from an inorganic power, namely the chemical forces entailed in respiration. This theme was developed greatly in the third edition of his *Human physiology*.

Turning to his discussion of the sexual functions, to discuss them in wholly dynamical terms as he did was quite unusual. There is a sizeable quantity of secondary literature on the history of ideas on sexuality, and although much of it has been researched meticulously, only a small proportion discusses the 19th century dynamical approach. Two studies which do this most usefully are Haller and Haller, *The physician and sexuality in Victorian America*,[34] and H.T. Engelhardt, 'The disease of masturbation: values and the concept of disease'.[35]

Carpenter's account of sexual activity was that the manufacture of seminal fluid required a large quantity of the body's energy, (an idea which goes back to Hippocrates at least),[36] that therefore each venereal act entailed a large expenditure of energy, and excessive indulgence would produce bodily exhaustion since the vital economy had only a finite stock of power which could be replenished only from its food. His, and his contemporaries', moral censure of promiscuity was thus given a valuable scientific boost which he and many others did not fail to exploit in their physiologies. Sometimes traditional ideas on sexuality were championed enthusiastically and uncritically; for instance, that seminal fluid was a discharge of the brain and spinal marrow.[37] Whatever the origins of 19th century discussions of sexual physiology, they almost all agreed that abnormal and excessive indulgence had one principal effect - debility, or over-consumption of the body's power. So said Carpenter:

> "The high degree of nervous excitement which the act of coition involves produces a subsequent depression of corresponding amount; and the too-frequent repetition of it is productive of consequences very injurious to the general health. This is still more the case with the solitary indulgence ... for this, substituting an unnatural degree of one kind of excitement for that which is wanting in another, cannot but be still more trying to the bodily powers. The formation of the seminal secretion itself seems to be a much greater tax on the corporeal powers, than might have been supposed *à priori*, and it is a well known fact that the highest degree of bodily vigour is inconsistent with a frequent indulgence in sexual intercourse; whilst nothing is more certain to reduce the powers, both of body and mind, than excess in this respect. These principles, which are of great importance in the regulation of the health, are but results of the general law, which prevails equally in the Vegetable and Animal Kingdoms, that the development of the individual and the reproduction of the species stand in an inverse ratio to each other".[38]

This general law was clearly a type of compensation principle. It was reiterated verbatim in the third edition of *Human physiology* (1846).[39] This was a revised edition and contained new discussions on the body's dynamics; for instance, sexual energy was correlated not only with other physiological forms of power but also with mental power;[40] the human animal was thus a versatile dynamical economy, in which the energy expended in any physical or mental activity would be a drain on the whole system. For instance, the sexual secretions, being strongly influenced by the mind, would be produced in greater or lesser quantity according to whether the mind contemplated lascivious or noble objects. Inevitably, however, sexual activity within marriage was considered salutary, so long as it was not excessive. On this point, all those who wrote about the physiological ill effects of sexual activity failed to deal satisfactorily; they failed to explain why a healthy adult could indulge vigorously during married life without debility, brain damage, neurasthenia etc., whilst even occasional extra-marital indulgence would be infallibly harmful. Carpenter tried to give a dynamical explanation why this should be so:

> "When the appetite is naturally indulged, that is, in marriage, the necessary energy is supplied by the nervous stimulus of its natural accompaniment of love, ... which prevents the injury that would otherwise arise from the increased expenditure of animal power: and in like manner, also, the function being in itself grateful, this personal attachment performs the further office of preventing immoderate indulgence...

> But, when the appetite is irregularly indulged, that is, in fornication, ... its energies become exhausted".[41]

A fine instance of how Carpenter used power in his physiology, and of how up-to-date the third edition of *Human physiology* was, occurred in his discussion of muscle contraction;[42] there, he discussed Helmholtz's research on the chemical changes which accompany muscle activity which Helmholtz did in 1845, and he was fully acquainted with Liebig's chemical hypothesis of animal heat. Informing his whole discussion was his evident belief in a correlation between organic and inorganic powers, for his conclusion from Helmholtz's work was that:

> "It cannot but be regarded as a probable inference from these facts, that the development of the Contractile Force is in some way dependant on the Chemical Change, which seems to be so essential a condition of it; just as the development of the Electric Force of the Galvanic battery is dependant on the new chemical arrangements, which take place between the bodies brought to act upon one another...."[43]

Another instance of a vital function depending upon an inorganic force was his view of the blood's purification; he used a curious argument - that since physical powers and laws are wholly mechanistic and determinate, whereas truly vital ones, if they exist at all, are indeterminate and fickle, it would have been grossly improvident for the removal of carbonic acid from the blood to be a vital process. Thus, the critical, purifying function of respiration was entrusted to simple physical laws.[44]

Carpenter's belief in the correlation of all powers seemed to free him from the necessity of vital forces to explain those functions that seemed to be confined to living organisms. Perhaps the best-substantiated case for him was animal heat, on which, having discussed the experiments of Lavoisier and Séguin, Liebig, Dulong and Despretz, he asserted confidently that:

> "Although the chemical doctrine of calorification cannot be regarded as yet perfected as to its details, there can be no reasonable doubt that it is altogether sufficient to account for the phenomena in question. And it may be stated as a general fact, that the production of Animal Heat is due to the various changes in chemical composition that are continually taking place within our system".[45]

Indeed, in these sections of his physiological text-book we see one of the earliest whole-hearted advocacies in Britain of Lavoisier's and Séguin's theory of respiration, in particular the dynamical implications that they themselves made in their first memoir on respiration in 1789. Let us recall that, having measured the differences in oxygen absorbed when a person eats, fasts, rests or works,they had concluded that oxygen usage was a measure of the equivalence of all forms of work:

> "This sort of observation permits us to compare the use of forces between which there might appear to be no relation. One could learn, for example, what weight in pounds corresponds to the efforts of a man reciting a discourse, or a musician

> playing an instrument. One could even evaluate how much there is of a mechanical nature in the work of a philosopher while reflecting, a man of letters while writing, a musician while composing. These efforts, ordinarily considered as purely moral have something physical and material about them which permits them to be compared, in this respect, with the efforts of a labouring man. It is then not without a certain rightness that the French language has confounded under the single designation work /travail/ the efforts of the spirit with those of the body, and work done in a study with that done in a shop".[46]

T.S. Hall has said of this passage that Lavoisier obviously possessed a prophetic intuition of what would be expressed half a century later as the conservation of energy.[47] With this interpretation I would hesitate, for the essential idea that Lavoisier and Séguin seem to have been expounding was an equivalence or correlation, a generic likeness, among different forms of power which would thenceforth permit a meaningful comparison between one power and another, no matter how different in form. Nonetheless, such an idea as theirs was, I suggest, an important stage in the development of ideas on power, for it is difficult to imagine the emergence of a conservation theory without the concomitant, or preferably prior, emergence of a correlation principle.

To return to Carpenter's discussion of respiration and allied topics, we find that the next two editions of his *Human physiology* contained little that was new, but the sixth edition (1864) offered important new ideas on power, and several ideas that had been expressed only vaguely in earlier editions were now boldly mentioned. Before discussing it, we must examine a paper he read before the Royal Society in 1850 'On the mutual relations of the vital and physical forces'[48] and the third edition of his *General and comparative physiology* (1851).

Historians might well consider the Royal Society paper as the high-water mark of Carpenter's physiological career. He himself thought it so, and one can see how his earlier, sometimes tentative, ideas on power now became explicit. Moreover, all editions of his *Physiologies* after 1850 differed notably from previous ones, for they incorporated the bold ideas of the paper. In the paper he acknowledged his debt to William Grove; Grove had expressed with unprecedented clarity since the early 1840s the ideas on power that Carpenter and others had been formulating for several years, and Carpenter felt the time was ripe to declare himself explicitly. In this respect, he considered Grove's theory of the correlation of physical forces not an innovation but rather a crystallization of ideas, and his task was to bring to fruition the physiological implications of that theory.[49] Actually, Grove himself had suggested the physiological import of his work, but Carpenter doubted that he had had in mind a correlation between the truly living functions (of growth, development and reproduction, for instance) and physical powers. To plumb these living functions was the task of a physiologist. Grove was not a physiologist; Carpenter was.

Carpenter saw his task much as Roget had, namely, not "to increase the knowledge of existing facts, so much as to develope new relations between those already known".[50] Just as Roget had hoped that some felicitous expression provided by the *Thesaurus* would open to his reader's mind a whole 'vista of collateral ideas', so did Carpenter hope that his method in physiology would "... open out a vast number of new lines of inquiry, which promise an ample harvest of results ...."

The greatest result that he envisaged from the paper was simply "... that Physiological science should be considered under the same *dynamic* aspect, as that under which the Physical sciences are now viewed by the most enlightened philosophers ....".[51]

This aim had for him the sanction of Lockean philosophy. Early in the paper, having outlined Grove's theory, he aligned himself with Locke's idea that all force, which does not emanate from the wills of God's creatures, proceeds directly and immediately from the Will of God himself; and therefore, regarding the so-called physical forces as so many *modi operandi* of one and the same agency, namely, the creative and sustaining Will of God, he could not see any validity in the objections that had been raised by several profound thinkers against the idea of the 'metamorphosis or conversion of forces'.[52] He referred especially to Chapter 21, 'On power', in Locke's *Human understanding*.

Locke was again enlisted towards the close of the paper. Starting with Locke's own notion of force, as emanating directly from the Divine Will, he proposed that such force, operating through inorganic matter, manifests itself in electricity, magnetism, heat, light, chemical affinity and mechanical motion; and that when directed through organized beings, it effects the processes of growth, development, chemico-vital transformations and the like, and is further metamorphosed, through the instrumentality of the structures thus generated, into nervous and muscular powers.[53]

Carpenter began his lecture with a historical review of physiology; there we see how he believed his view of the relation between vital functions and physical forces differed from the general view of recent physiologists; whereas they had envisaged those forces acting only as stimuli on the vital system and rarely participating in its living, internal dynamics, he was seeking a more intimate relation between them. He thought himself alone in his approach, for he had never found

> "... in physiological writings any indication of a more intimate relationship between the physical forces and vital phenomena, than that just stated - save on the part of those who have vaguely identified Heat or Electricity with the 'vital principle', with about the same amount of philosophical discrimination as that which was exercised by the iatro-chemists and iatro-mathematicians of the sixteenth and seventeenth centuries".[54]

The next section of the paper sought mutual relations among the vital functions. He discussed evidence for the truly vital nature of certain plant and animal functions, vital functions being those which seemed to occur only in living systems and whose complexities put them beyond the reach of simple physico-chemical explanation. This distinction seemed valid, although he would not accept any peculiarly vital force as its basis: it was the functions, or the behaviour of organisms, which were vital, but not the powers which effected those functions.[55] One of the clearest instances of such vitality seemed to him the movement of fluids - a field that men like John Hunter, William Alison, Charles Bell and John Draper, among others had also considered central to the study of life. He discussed Alison's study of the blood, agreeing with him on its essentially vital behaviour but disagreeing with his hypothesis of vital forces.[56]

The next task was to show that the different causes or forces effecting such functions were correlated with one another. One line of evidence arose from Schwann's cell theory, for all forms of vital force were exerted through a common instrumentality, the cell; and all the cells, no matter how differentiated, in any organism were descended from a common ancestral cell; moreover, in single-celled organisms all forces and functions were known to occur without any apparent specialization. All living forces could therefore be expressed collectively as 'cell force', each different function being the effect of a modified form of that force. Carpenter was quick to point out that by cell-force he was not raising the spectre of yet another definitive but incomprehensible vital power; the use of that expression

"... is just that which is commonly made of the term 'Engine-power'; everyone knowing that the steam-engine possesses no power itself, but that it is simply the instrument most commonly exployed, because the most convenient and advantageous yet devised, for the application of the expansive force of steam, generated by the application of heat, to the production of mechanical motion".[57]

This cell-theory discussion led into a dynamical version of the principle of compensation although he did not mention Paley or the principle explictly. There was abundant evidence that when specialization of function occurred in the formation of higher organisms, the cells which effected one particular function lost their capacities for other functions. Thus the assimilating cells, which converted raw foodstuffs into 'organizable plasma', were observed to do scarcely any purely chemical transformation; they could not exert mechanical or nervous power or reproduce themselves. Carpenter concluded that the expenditure of the vital force of any cell upon a specific function seemed to unfit it for any other function.[58] This theme in his *Human physiology* entailed also a discussion of the debilitating effect of sexual indulgence; so too in his paper, though with a more confident assertion of the reciprocity or correlation of vital powers:

"That a relation of reciprocity exists between the forces concerned in the growth, development and maintenance of the individual organism, and those which are employed in the generative act - so that an excessive expenditure of either diminishes the amount of vital force

> which is applicable to the other - is an idea so familiar to physiologists that the author need not here dwell upon it, further than to point out how completely it coincides with, and illustrates, the view for which he is contending".[59]

The correlation theory accounted neatly for other phenomena he had discussed in his *Physiologies*, to wit the relation of nerve power to muscle contractility; on this he cited especially the Italian physiologist, Carlo Matteucci (1811-1868), who had also enquired into the relation between physical and vital forces; he praised Matteucci's widely-read *Lectures on the physical phenomena of living beings* (1847),[60] which tended to support his own ideas in dynamical physiology.

Carpenter concluded this section by stating clearly what the correlation theory meant for the purely vital functions: that so close a mutual relation existed between all vital forces that they should be regarded as modes of a common cell force. Moreover, although this was not explicit, his argument implied a conservation, or at least a non-creation, of vital power; for only by so envisaging the organism could his assertion of its exhaustion following overactivity (of the sexual functions for instance) have any useful meaning. Indeed, there would have been no point in constructing a correlation theory for vital forces as rigo~~u~~rously as he tried to do, (remember that his was no idle speculation, but was based on the physical researches of Grove), and there could not have been any hope of testing that theory, if he had not envisaged the organism as a non-creator of power, just as most natural philosophers already envisaged physical machines. Although there was no explicit assertion of a conservation of power or force anywhere in the paper, Section 3 developed along that line.

Section 3 was entitled 'Relations of the vital and physical forces'. The most obvious field to seek correlations was nerve activity, for physiologists had been toying with the idea of an essential identity between that and electricity for at least a century, ever since the discovery of the Leyden Jar; the work of Galvani, Volta, Davy and others had provided support for the idea and some fine experimental studies had been done on it throughout Europe. Carpenter cited Matteucci's research which had apparently disproved an identity, yet he believed that there was abundant evidence for a correlation or mutual convertibility: electricity, when acting through nerve-fibres, developing nerve force; and nerve force, when acting upon special apparatus, developing electricity.[61] He cited Johannes Müller's principle of specific nerve sensations, (to be discussed in Chapter 13), which agreed well with the correlation theory and which suggested to him that the relation between electricity and nerve-force was analogous to the relation between electricity and magnetism; nerve force was the effect of electricity in a nerve, magnetism being the effect of electricity in a piece of iron.[62] The experiments of Davy, Faraday and Matteucci on electric fishes were cited, which had shown that the electric power generated was "in precise accordance with the amount of nervous force which is transmitted".[63] In Carpenter's opinion, therefore, there was not only a qualitative connexion but also a rigorous quantitative one, namely, a conservation of the net nervous and electric power during their interconversions.

He was pleased to add that since he had written his paper he had found Matteucci adopting the doctrine of correlation between nervous and electric forces in his eigth series of *Electrophysiological researches* (1850).[64] He added that in a review[65] of Matteucci's *Lectures* (1848), which he had written in 1848, he had asserted the correlation principle himself. In a long footnote on the first page of the Royal Society paper he quoted from that review:

> "There can be no doubt that the present tendency of scientific investigation is to show a much more intimate relation than has been commonly supposed to exist between vital and physical agencies; and to prove that, whilst the former are of a nature altogether peculiar, they are yet dependent upon conditions supplied by the latter. And the more closely these phenomena are investigated, the more intimate and uniform does that dependence appear; so that we seem to have the general conclusion almost forced upon us, that the *vital* forces of various kinds bear the same relation to the several *physical* forces of the inorganic world, that they bear to each other; the great and essential modification or transformation being effected by their passage, so to speak, through the germ of the organic structure, somewhat after the same fashion that heat becomes electricity when passed through certain mixtures of metals".[66]

In the same footnote he cited a paper written by Dr. Richard Fowler in 1849, entitled 'If vitality be a force having correlations with the forces, chemical affinities, motion, heat, light, electricity, magnetism, gravity, so ably shown by Professor Grove to be modifications of one and the same force?'[67] Clearly, Matteucci and Fowler were working along the same line as Carpenter, though he was apparently unaware of their correlation ideas until 1850.

Returning to Section 3, Carpenter discussed how physical forces other than electricity were correlated with nerve force,namely heat, chemical affinity, light and motion. On motion he cited Müller's *Physiology* and again made a conservation of power assertion: that the proportionality between motor force and the nervous power exerted would be admitted by all physiologists, the most suasive illustration being the extraordinary force which is often developed by persons under emotional stress.[68] The least substantiated correlation was between nerve force and magnetism, on which he would not commit himself except to cite the researches of Baron von Reichenbach and Faraday which indicated a likely connexion. Priority was accorded to von Reichenbach, (who had devoted much time and money to an extensive and careful investigation. He is discussed in a later chapter).

The next theme was the possibility of correlations between vital forces other than nerve agency, and the physical ones. Muscle force was an obvious choice since it was well known that it was associated with the flow of electricity to or from muscles. On this issue, a lot of literature had appeared already and much has been written by medical historians. To cite only one historian, Dorothy Needham,

> "Many observations were recorded from the 17th century onwards; indeed, Volta in 1800 defined his Pile as an artificial electric organ. We may also recall the triumphant letter of Walsh to Benjamin Franklin in 1773: 'It is with particular satisfaction that I make my first communication, that the effect of *Torpedo* appears to be absolutely electrical'".[69]

Carpenter adopted Liebig's recent explanation of muscle force; it summarized the whole endeavour of these two vital materialists so well, that Carpenter's words merit extensive quotation:

> "These agencies [electricity, heat etc.], however, do not appear so directly concerned in the production of the motor power, as in occasioning that metamorphosis of living, organized tissue into chemical compounds, whereon the development of the muscular force seems to be immediately dependent. It is now universally admitted that the disintegration of a certain amount of muscular tissue, and the new arrangement of its components in combination with oxygen supplied by the blood, is necessary for the development of its contractile force; and the considerations adduced by PROFESSOR LIEBIG render it highly probable, that the muscular contraction may be regarded as proceeding from the expenditure or metamorphosis of the cell-force, which ceases to exist as a *vital* power, in giving rise to *mechanical* agency. The amount of muscular force developed appears to bear an exact correspondence with the amount of urea formed by the metamorphosis of the muscular tissue; and this metamorphosis

> involves the cessation of its existence as a living structure, and consequently the annihilation of the vital forces which that structure possessed. We are, then, to regard the nervous, electrical and other stimuli, under whose influence the muscular force is called forth, less as the immediate sources of that force, than as furnishing the conditions under which the vital force, acting through the muscle is converted into the mechanical force developed in its contraction".[70]

Carpenter considered it easy to correlate the forms of vital force mentioned so far with physical forces; a greater challenge was to show how physical forces were implicated in the growth and development of living beings. Most historians of late 18th - early 19th century biology would agree with Driesch and Wheeler[71] that the study of development tended to develop vitalistic beliefs, whether one was an epigeneticist or a preformationist, (although epigenesis was perhaps the more likely road to vitalism). Against the vitalistic theories of embryological evolution and growth that had been enunciated in the past 80 years or so,[72] particularly on the Continent, Carpenter had to contend. One of the primary pieces of evidence for his correlation view was the obvious fact that all life seemed to depend on heat and/or light. The question was whether that dependence was total or only partial. The boldest hypothesis for a total dependence had been advanced by Jean Baptiste Boussingault (1802-1887), the French chemist and physiologist, and Carpenter cited him eagerly. According to Boussingault, the same species of plant in passing through its several phases of life from germination to death, receives the same quantity of sunlight and heat, whether it be grown at the equator or the temperate zone or anywhere else; its rate of growth is in a precise inverse ratio to the quantity of light and heat it receives, and its organizing force is equivalent at all times to its imbibed solar power.[73] Carpenter, though not in total agreement with Boussingault, believed that his and other men's studies had shown that heat was more than a mere stimulus upon an organism, and that it manifested itself as vital force whenever it operated within an organized structure. This idea, he added, "accords with the fact of the restoration to the inorganic world - under some form or other - of all the *force* thus withdrawn from it".[74] Clearly, the inorganic and organic realms together constituted a conservatory of power or force.

Carpenter believed this view differed significantly from those of almost all other physiologists, a difference especially evident in their accounts of embryological evolution. This difference had at its root Grove's correlation theory as applied to physiology:

> "According to the doctrine current among some physiologists, the whole 'organizing force', '*nisus formativus*', or '*bildungstrieb*', which is to be exerted in the development of the complete structure, *lies dormant in this single cell*, the germ (it has been affirmed) being 'potentially' the entire organism. And thus all the organizing force required to build up an oak or a palm, an elephant or a whale, is concentrated in a minute

particle, only discernible by microscopic aid.

As a refuge from this doctrine, ... other physiologists, (among whom the author formerly ranked himself), have affirmed that vital force must exist in a dormant condition in all matter capable of becoming organized; that the germ cell, in drawing to itself organizable materials, and in incorporating these into the living structure, does nothing else than evoke into activity their latent powers; and thus that, with every act of growth and cell-multiplication, new vital force is called into operation, whereby the process is continually maintained. This proposition does not involve any manifest absurdity. It attributes to oxygen, hydrogen, carbon and nitrogen properties which they were not previously supposed to possess; but no-one else could logically deny to these elements the possession of dormant vital powers, whilst they held that a dormant magnetic power might be attributed to iron ....

The views of PROF. GROVE, however, strike at the root of the notion of *latent force* of any description whatever; all force once generated being, in his estimation, perpetually *active* in one form or another; and its supposed "latency" being a hypothetical condition, the idea of which is quite unnecessary when the force which has ceased to manifest itself is recognized under some other form. Thus, in his view, when iron is rendered magnetic by an electric current, the development of the magnetic force is rather to be looked on as the result of the conversion of the electric, by the instrumentality of the iron, than as a case of the excitation of one force previously dormant,by another which is expended in thus evoking it. Such an analogy should rather lead the physiologist to look for some extraneous source of the organizing force; and to suspect that when organizable materials are applied to the extension of a living structure, and are caused to manifest vital forces, some agency external to the organism is the moving spring of the whole series of operations. And thus, according to the view here advocated, the vital force which causes the primordial cell of the germ first to multiply itself, and then to develop itself into a complex and extensive organism, was not either locked up in that single cell, nor was it latent in the materials which are progressively assimilated by itself and its descendants; but is directly and immediately supplied by the Heat which is constantly operating upon it, and which is transformed into vital force by its passage through the organized fabric that manifests it".[75]

Heat was not the only physical agent known to be essential for organic development. He cited W.F. Edwards' experimental study of the influence of light upon the metamorphosis of batrachia,[76] and a recent study by Jörgen Christian Schiödte, the Scandinavian naturalist, of the non-development of eyes in subterraneous creatures like the *Proteus anguineus* and the *Amblyopsis spelaeus*.[77] On the influence of electricity he again cited Matteucci.

In the paper's concluding paragraphs Carpenter gave a fine account of his vision of the world: there was a continuous interchange and metamorphosis of both matter and force between the inorganic and organic realms; the world was treated as a closed system within which the conservations of matter and force were the guiding principles which enabled the natural philosopher to have a clear view of the unity of Nature. It is easy to see the philosophical kinship between Carpenter and men like Hutton, Davy and Roget; through their own specialized and painstaking studies, they believed they could construct comprehensive and rigorous views of Nature's unity. Carpenter's view was as follows: Not only is there a continuous material cycle between the inorganic and organic realms, but all the *forces* , which are operative in producing the phenomena of life, are in the first place derived from the inorganic universe, and are finally restored to it.[78] In plants, light becomes metamorphosed into chemico-vital affinity, and heat manifests itself as vital force. The organic products of the working of light and heat within the plant become the food of animals and thus produce nervous and muscular forces. Taking his cue explictly from Liebig, the vital force, which effects primarily growth in plants, effects primarily the nervous and muscular forces in animals, whilst the organizing force requisite for these animal processes is heat. During animal life, therefore, there is a continuous restoration to the inorganic world of carbonic acid, water and ammonia, the quantities being exactly proportional to the heat and motion generated by the animal and the material it consumes as food. For Carpenter, this cycle was primarily dynamic:

> "So that, on the whole there is strong reason to believe that the entire amount of force of all kinds (as of materials) received by an animal during a given period, is given back by it during that period, his condition at the end of the term being the same as at the beginning. And all that has

> been expended in the building up of
> the organism, is given back by its
> decay after death".[79]

In his paper we can see how several ideas which had appeared in his earlier works now became explicit; he drew attention to this in a footnote at the beginning where, having cited Fowler's correlation paper, he claimed priority for himself since the subject had been occupying his attention for some years; his paper was the first systematic treatment of it and was essentially a re-presentation of his own ideas in the light of new evidence and within the context of a newly stated physical principle. His mentors were mainly Grove and Liebig. Interestingly, not once in the paper did he mention Helmholtz, even though he discussed that aspect of muscle contraction to which Helmholz's studies in the mid 1840s contributed so much. Apparently, he was unaware of Helmholtz's 'Über die Erhaltung der Kraft ' paper, which was likely enough since that paper was relatively little known even in Germany and had not been translated into English. The man who most influenced him on the conservation of power was therefore Liebig, for his name and work had acquired fame already in England.

One more comment on Carpenter's paper: he was in no doubt about the difficulty of proving an hypothesis in physiology, that the life-sciences are not as amenable to rigorous investigation as the physical sciences, and that all broad physiological arguments are necessarily cumulative. He emphasized that the cunulative argument of his paper was what counted, and not the validity of each point.[80] Indeed, such has been the argumentative method in the life-sciences from Aristotle to Darwin, perhaps even from the origin of human thought to the present day.

The cumulative argument was developed next in the third edition of his *General and comparative physiology* (1851).[81] In the preface he specified his own contributions thitherto to physiology; pride of place went to his work on 'the mutual connection of the vital forces, and their relation to the physical', and fourth in the list was his application of Von Baer's law of development from the general to the special.[82]

On the use of power in this edition, four points are notable.

1) He was much more explicit than in earlier editions.

2) He discussed more eagerly the theological implication of the correlation and conservation theories, and was much concerned with the philosophical validity of 'force' and 'laws of Nature'; in this, he followed and quoted Locke. By force he meant only 'affections of matter' or the direct operation of the primal, all-sustaining First Cause. Vital forces were affections of matter in particular states of organization. So far as human understanding could reach, they were ultimate facts in physiology, just as attractions and repulsions were ultimate facts in physics, and affinities in chemistry. The constancy of their actions indicated the constancy of God's Will.[83]

3) He discussed more eagerly than thitherto the primary rôle of the Sun and its light in the dynamical system of the world. One likely reason for this is that he had read and was quoting John Draper's recent treatise on *Forces*

which produce the organization of plants.[84] It is doubtful that he had read it before writing the second edition of his *Physiology*. Now, like Draper, he wrote lyrically on sunlight[85] and quoted Lavoisier's powerful passage on it. In view of his geological interest, he might have read one or more of Hutton's treatises on natural philosophy, from which he would have obtained further ideas on the paramounce of light; but for this I have no evidence.

4) He explicated respiration in bold dynamical terms and proposed more confidently that the bodily tissues are the *loci* of respiration. Respiration was necessary because of the energetic exertion of an animal's powers and its requirement for a high temperature. Apparently, this view had been championed in a paper he wrote as long ago as 1835 for the *West of England Journal*, and he was pleased to note that it had been confirmed by subsequent researchers, particularly Gustav Magnus (1802-1870) and was now generally accepted.[86]

The next step in his discussion of power was an essay on 'The phasis of force' (1857),[87] a metaphysical as well as scientific work, in which he developed slightly differently than before the physiological application of the correlation theory; again, there was no explicit, separate mention of conservation, but it was strongly implied. A large proportion of the essay was given to Liebig's chemico-physiological ideas on power, and the following synopsis of the essay might well be a synopsis of whole chapter of Liebig's *Animal Chemistry*: Carpenter suggested that, apart from animal heat, the most characteristic method whereby animals restore force to the universe is by their motion. Motion being an expression of force, he asked after the source of that force, answering that most physiologists now agreed in the principle first formally stated by Liebig, that every muscle contraction involves the death and oxidation of a quantity of muscle tissue proportional to the force exerted. The motion produced by each contraction could be regarded as an expression of the vital force which is superceded by chemical action, and as having the same relation to that chemical action which a voltaic current has to the oxidation of zinc in a battery.[88] As Liebig first proposed, the nitrogenous constituents of plants and animals possess large concentrations of chemical force which, on being released, manifest themselves as ferments, motion, heat and other physiological phenomena. Just as the idea that death and oxidation of living tissues, provided the organism with its power for heat and motion had been a cornerstone of Liebig's physiology, so too was it for Carpenter. Thus, for nervous power:

> "... its source lies, like that of muscular power, in the chemical changes involved in the death and decomposition of the peculiar tissue which manifests it".[89]

In this essay and previous writings, Carpenter admitted the reality of matter as well as of force. In this, he departed from Boscovich and Exley and was part of the mid-century movement in European physiology which might be described as the 'materialistic metaphysic' in which force and matter were the ultimate realities in physiology as in natural philosophy. However, he attributed more certainty to force than to matter, and (it seems to me) the main purpose of his 'Phasis of force' was to argue this point. Why else would he write as follows?

> "We shall have greatly failed in our purpose, however, if we have not by this time led our readers to perceive how complete is the distinction between *matter* and *force*, and how close is the relation between *force* and *mind*. Matter is in no case more than the embodiment or instrument of force; all its so-called active states being merely the manifestations of an energy which, under differet forms, is unceasingly operative. Nor can it be fairly said, that in substituting the doctrine of force for that of the 'imponderables', we are only setting up one hypothetical entity in place of another. Force is truly more of a reality to us than matter itself; for we cannot become cognizant of even the most fundamental property of matter - its occupation of space - without the consciousness of resistance.... And as we are thus led by the 'correlation' doctrine to consider the various agencies of nature as the expression of a conscious will, we find the highest science completely according with the highest religion, in directing us to recognize the omnipresent and constantly sustaining energy of a personal Diety in every phenomenon of the universe around us ...."[90]

Once again, his general argument implied a conservation of power, such conservation being guaranteed by God's sustaining energy.

In the sixth and much revised edition of his *Human physiology* (1864)[91] the conservation of energy was at last discussed explicitly. This edition had been edited by his physiologist friend, Henry Power (1829-1911), who had rewritten almost the entire book in order to keep down its length. But the first two chapters were Carpenter's original composition; and Chapter 1, 'Of life and its conditions',[92] was nothing but the physiological application of the correlation and conservation theories, in which his earlier ideas were now explicit. He referred to those philosophers, besides himself, whom he considered to have made original and seminal contributions, namely Liebig, Mayer, Grove and Joule. Again, Helmholtz was not cited as an original contributor, though he was mentioned as a subsequent participant in the debate.[93]

Carpenter arranged the current views of life into three types:

1) Life as an aggregate of living phenomena as they appear to us, without any ideas about its mechanism.

2) Life as a mode of activity unique to living beings.

3) Life as a special vital agency or power inherent in each organism.

The first type, held by the 'positive' school, (of whom Draper was one), he rejected since it ignored the forces which operate the organism; it was as ridiculous as if, in studying the operations of a cotton factory, one restricted one's interest to the mechanism of carding, weaving and spinning whilst ignoring the motive power without which all the machines would be inert.[94]

The third type was rejected since it was incapable of being tested experimentally and was therefore useless; it was as speculative as explaining the transformation of raw cotton into woven calico by the agency of a 'calico-making principle'.[95]

The second type was most useful since it accorded with recent physical researches, and the physiologist could discern thereby the perpetual dynamic cycle and unity of Nature.

In discussing the source of nervous and muscular forces he appended two footnotes. One enumerated the contributors to correlation and conservation, explicitly mentioning conservation as a distinct principle; the other mentioned later, less original contributions by Helmholtz and William Thomson (1824-1907). The text of the essay proper argued that animal existence involves a constant expenditure of motor force, heat and, in the case of man, psychic powers, and that these derive from the chemical force stored up in the body's tissues and food.[96] Again, Liebig was acknowledged as the originator of this theory, although Carpenter thought that his ideas were being superseded:

> "... he seems to have regarded the motor force produced as the expression of the vital force by which the tissue was previously animated; and to have looked upon its disintegration by oxygenation as simply a consequence of its death. The doctrine of the 'correlation of forces' being at that time undeveloped, he was not prepared to recognize a source of motor power in the ulterior chemical changes which the substance of the muscle undergoes; but

> seems to have regarded them as only concerned in the production of heat. The earliest distinct expression of the current doctrine is to be found in the very remarkable treatise of Dr. Mayer, in which he worked out from the two fundamental axioms, "*Ex nihilo nil fit*" and "*Nil fit ad nihilum*", the whole system of doctrine which has since come to be known as that of the 'correlation of forces' and the 'conservation of force', in its application alike to physics and chemistry and physiology. Prof. Grove was simultaneously engaged in the development of the doctrine of the 'correlation of the physical forces'; and without any knowledge of Dr. Mayer's previous labours, the author of this treatise developed the doctrine in the form stated in the text, in his memoir 'On the mutual relations of the vital and physical forces', published in the *Philosophical Transactions* for 1850".[97]

This passage is important for the historian since it reveals the extent, as well as the limitations, of Liebig's contribution in Carpenter's opinion. It also shows that Carpenter considered the primary principle to be correlation, with conservation only a particular aspect of it, whose development from mainly metaphysical premises was due to the thitherto unknown genius of Mayer. This supports my earlier contention that Carpenter's concept of the correlation of forces entailed a rough awareness of the conservation principle, and that a rigorous correlation theory only makes real sense if there is an implicit acceptance too of a conservation, or at least a non-creatibility, of force. Carpenter seems to have thought that the two principles of correlation and conservation of force are mutually inclusive and that, when considered carefully, they are two sides of the same coin. Only thus can we explain his general lack of enthusiasm for conservation as a separate principle, for he thought it only a slight modification of the more substantial principle of correlation; in his opinion, correlation had been the subject of careful, extensive experimentation and reflection by eminent men like Liebig, Grove and Joule, not to mention himself; whereas conservation was only the brainchild of an ingenious metaphysician - albeit a remarkable brainchild.

That both correlation and conservation of force were central features in his physiology, with correlation the more useful, can be seen from his summary of Chapter 1, where he described human life as essentially the manifestation of different forces which are developed by the "retrograde metamorphosis of the organic compounds generated by the instrumentality of the plant".[98] Even the reproductive force was of the same origin, albeit in an especially concentrated form.

The last of Carpenter's published writings that shall be discussed in this thesis is his Presidential Address to the British Association for the Advancement of Science in 1872, entitled 'Man the interpreter of Nature'.[99] Departing from the custom for the president to review recent progress in a particular scientific field, he chose instead to deliver a general philosophic lecture whose main concern was to oppose the growing belief that, through science, man could understand Nature entirely and could construct prescriptive laws for her. Such a reaction against the ever-growing arrogance of science was not new to the 19th century; as Basil Willey[100] and other historians have shown, it had been going on since the beginning of the century, to wit Coleridge and Newman among others. What is interesting about Carpenter's reaction was that it arose from his own scientific background and was based on his own first-hand acquaintance with methodological and philosophical difficulties, particularly within his own science of physiology. Total confidence in science, he said, is naïve since it ignores the difficulties inherent in sense-perceptions, intuition, logic and human judgement. He distinguished two types of scientific law; one was only a generalization of phenomena and was therefore not a proper law - such were the so-called laws of chemical combination; the other was a true law because it described the effects of a force, and force was an ultimate fact, even more certain than matter - the laws of gravitation and energy conservation (which he attributed solely to Mayer) were therefore real laws.[101]

Since the only certainties in natural laws were the powers behind them, man's greatest task was to contemplate those powers; Carpenter's own contemplation led him to appreciate the unity of Nature and God's ever active power. Consequently, in the following extract we see the natural philosophy of a well-informed physiologist, a man of science, whose guiding light had always been the dynamics of Nature; no better words than his own can convey the import of power for his world view:

"To speak of any law as regulating or governing phenomena is only permissible on the assumption that the law is the expression of the *modus operandi* of a governing power ....

And thus we are led to the culminating point in man's intellectual interpretation of Nature - his recognition of the unity of the power, of which her phenomena are the diversified manifestations. Towards this point, all scientific inquiry now tends. The convertibility of the physical forces, the correlation of these with the vital, and the intimacy of that *nexus* between mental and bodily activity ... all lead up to one and the same conclusion".[102]

That conclusion was the unity of Nature, on which the brilliant lines of Pope were quoted:

"All are but parts of one stupendous whole,
Whose body Nature is, and God the Soul."

As the closing words of his lecture show, Carpenter shared Pope's vision in all its glory:

"For whilst the deep-seated instincts of humanity and the profoundest researches of philosophy alike point to mind as the one and only source of power, it is the high prerogative of science to demonstrate the *unity* of the power which is operating through the limitless extent and variety of the universe, and to trace its *continuity* through the vast series of ages that have been occupied in its evolution".[103]

Summary

It is difficult for an historian to evaluate the work of a man like Carpenter, for most historians of physiology and most historians of the correlation and conservation of energy have neglected him; only occasionally is he acknowledged as an original contributor to the energy debate, as in J.E. Carpenter's selective edition of his work, *Nature and Man*.[104] Yehuda Elkana does not mention him in his recent book, *The discovery of the conservation of energy*,[105] but that is scarcely surprising since he does not seem to have intended to do any research on the possibility of a physiological contribution.

This chapter on Carpenter has therefore been a much-needed, detailed rehabilitation of him as an original and thorough contributor to the development of the correlation and conservation principles. I have discussed his work into the 1870s, for only thus could it be shown that his early ideas in dynamics - as they appeared in his physiological writings in the 1830s and early 1840s - were more than vague speculation and did constitute a roughly-hewn but definite conception of the correlation of Nature's powers. Moreover, as his physiological textbooks were revised and as new evidence came in from the Continent, we see that his correlation theory became more rigorous and also resulted in the explicit statement of conservation of energy, which had been only implicit in his earlier work.

If we try to evaluate his standing among his contemporaries - which is an essential task for an historian - we must admit that he was a well regarded physiologist, whose work impressed a fair number of his equals and peers. A persuasive instance of this is a letter written to him by the elderly Mary Somerville, congratulating him on a lecture-series he had given at the Royal Institution in 1859-60 on 'The relation of the vital to the physical forces':

"Florence, June 12,1860.

Dear Dr. Carpenter,

The proof of the sequence of forces by which you have connected mind with mind, and transmitted your ideas to the minds of your audience, has required a higher power of intellect than that of making electricity the bearer of thought from continent to continent, sublime as it is, inasmuch as many intellects were combined to effect the latter, while the general tendency of science seems to have been your sole guide in demonstrating that matter is merely the medium through which mind-force, like all other force, acts, and that thus mind may, and does in fact, exist independently of matter. The series which you have completed is very beautiful. First, Mr. Grove's masterly demonstration of the correlation of the physical forces, then your proof of their correlation with the vital force so happily illustrated by the zoospores, and lastly the remarkable correlation between the vital and mental forces. No doubt, this series will mark the middle of the 19th century as a great scientific epoch ...."[106]

If Somerville, Paget, Huxley and others thought so well of Carpenter's work, why have historians of 19th century science largely ignored him? The reasons seem to me to be similar to those for Roget's neglect. Carpenter's was not a powerfully original or imaginative mind; it preferred sobriety and evidence to flashes of intuition. Like Roget, he preferred writing to research, the dispersal of scientific knowledge to the many to the intensification of knowledge among the few. Like Roget, he was both enquiring and didactic, with the didactic taking up much of his energy and time. Had they written less and concentrated more on their enquiries they might well have carved themselves more noticeable niches in history. However, history should not be concerned merely with notabilities, and when fine minds like Roget's and Carpenter's contributed to the emergence of an important new aspect of man's world view they ought not to be ignored.

NOTES TO CHAPTER 7.

1. Anon., *List of the graduates in medicine in the university of Edinburgh, from MDCCV to MDCCCLXVI*, Edinburgh, Neill & Co., 1867. "Guilielmus Benjamin Carpenter, Anglus. On the physiological inferences to be deduced from the structure of the nervous system in the invertebrated classes."

2. J.E. Carpenter, *Nature and Man, essays scientific and philosophical by William B. Carpenter*, London, Kegan Paul, Trench & Co., 1888.

3. W.B. Carpenter, 'On the structure and functions of the organs of respiration in the animal and vegetable kingdoms', *West of England J.*, Oct. 1835 and Jan. 1836, cited in *ibid.*, p.467.

4. W.B. Carpenter, 'On the voluntary and instinctive actions of living beings', *Edinb.med.surg.J.*, 1837, 132, cited in *op.cit.*, Note 2 above.

5. W.B. Carpenter, 'On unity of function in organized beings', *Edinb.new phil.J.*, July 1837, cited in *op.cit.*, Note 2 above.

6. *Op.cit.*, Note 2 above, p.64.

7. *Ibid.*, pp.64-65.

8. *Ibid.*, pp.66-67.

9. W.B. Carpenter, *The microscope*, London, Churchill, 1856. This went through several editions during his life and was as popular in the U.S.A. as in Britain.

10. *Op.cit.*, Note 2 above, pp.78-81.

11. T. Exley, *Principles of natural philosophy, or a new theory of physics*, London, Longman, 1829.

12. D.M. Knight, *Atoms and elements*, London, Hutchinson & Co., 1967, pp.65-70.

13. *Op.cit.*, Note 11 above, p.473.

14. *Ibid.*, p.3.

15. T. Exley, 'On the laws of chemical combinations and the volumes of gaseous bodies', *Rep.Brit.Ass.Advmt.Sci.*, 1848, pp.50-51 of transactions of the sections.

16. Cited in J.E. Carpenter, *op.cit.*, Note 2 above, p.20.

17. W.B. Carpenter, 'Physiology an inductive science', *Brit.* and *For.Med.Rev.*, April, 1838, cited in *ibid.*, pp.156-157.

18. *Ibid.*, pp.157-158.

19. W.B. Carpenter, *Principles of general and comparative physiology*, London, J. Churchill, 1839.

20. W. Paley, *Natural theology*, 12th edition, London, J. Faulder, 1809, pp.275-276.

21. *Op.cit.*, Note 19 above, p.299.

22. T.S. Hall, *Ideas on life and matter*, vol.2, Chicago, University of Chicago Press, 1969, pp.223-228.

23. *Op.cit.*, Note 19 above, pp.382-383.

24. Cited in *ibid.*, p.384.

25. *Ibid.*, pp.382-383.

26. *Ibid.*, p.384. The empirical, and even experimental

evidence for these electrical implications in human temperament etc., was thoroughly respectable, and it seems to me to have been almost inevitable that an up-to-date and critical physiologist like Carpenter would have considered it an issue of great promise for future research.

27. *Ibid.*, p.463.

28. W.B. Carpenter, *Principles of general and comparative physiology*, London, J. Churchill, 1841.

29. W.B. Carpenter, *Principles of human physiology*, London, J. Churchill, 1842.

30. *Ibid.*, p.609.

31. *Ibid.*, p.609. For a useful discussion of the nervous theory of respiration, see the two references below.

32. E. Mendelsohn, *Heat and life, the development of the theory of animal heat*, Cambridge,Mass., Harvard University Press, 1964.

33. G.J. Goodfield, *The growth of scientific physiology*, London, Hutchinson, 1960, Chapters 1,2 and 4. Carpenter is not mentioned in this book. However, so scholarly and thorough is it, that had Goodfield intended a larger, more comprehensive survey I cannot doubt that Carpenter would have been cited for the lengthy and careful reviews that he made of respiration theories.

34. J.S. Haller & R.M. Haller, *The physician and sexuality in Victorian America*, Urbana, University of Illinois Press, 1974.

35. H.T. Engelhardt, 'The disease of masturbation: values and the concept of disease', *Bull.Hist.Med.*, 1974, 48:234-248.

36. *Op.cit.*, Note 34 above, Chapter 5.

writer Aulus Cornelius

37. The Roman ~~physician~~, ~~Aurelius~~ Celsus (fl.ca.A.D.25), wrote of the unnatural or involuntary losses of semen as a wasting of the spine, an opinion which became popular in the rural communities of England in the 19th century. Hippocrates wrote in similar vein that "the humours enter into a sort of fermentation, which separates what is most precious and balsamic; and this part, thus separated from the rest, is carried by the spinal marrow to the generative organs." Hippocrates used the term '*tabes dorsalis*' (spinal consumption) to describe the evils attending the unnatural emission of semen. The disease was supposed to originate in the spinal marrow and attacked young men whose loose characters led them to sexual excesses. In Western physiology during the 19th century, such ideas were often taken straignt from the Greeks. For a discussion, see *op.cit.*, Note 34 above, Chapter 5.

? Source Haller Haller p.195

38. *Op.cit.*Note 29 above, pp.620-621.

39. W.B. Carpenter, *Principles of human physiology*, 3rd edition, London, J. Churchill, 1846.

40. *Ibid.*, p.475.

41. *Ibid.*, p.475.

42. *Ibid.*, pp.440-442 especially.

43. *Ibid.*, p.441.

44. *Ibid.*, p.580.

45. *Ibid.*, p.698.

46. A Lavoisier and A. Séguin, 'Premier mémoire sur la respiration des animaux' (1789), in *Oevres de Lavoisier*, Paris, Imp.Roy., 1864-1893, 2:697.

47. T.S. Hall, *op.cit.*Note 22 above, p.163.

48. W.B. Carpenter, 'On the mutual relations of the vital and physical forces', *Phil.Trans.*, 1850, pp.727-757.

49. *Ibid.*, P.730.

50. *Ibid.*, pp.756-757.

51. *Ibid.*, p.757.

52. *Ibid.*, p.730.

53. *Ibid.*, p .752.

54. *Ibid.*, p.729.

55. *Ibid.*, pp.728-729.

56. *Ibid.*, p.736.

57. *Ibid.*, p.737.

58. *Ibid.*, p.738.

59. *Ibid.*, p.739.

60. C. Matteucci, *Lectures on the physical phenomena of living beings*, translated by J. Pereira, London, Longman et al., 1847.

61. *Op.cit.*, Note 48 above, p.742.

62. *Ibid.*, p.743.

63. *Ibid.*, p.744.

64. C. Matteucci, *Traité des phénomènes électro-physiologiques des animaux*, Paris, Fortin, Masson & Cie., 1844.

65. W.B. Carpenter, 'Dr. Pereira's edition of Matteucci's Lectures', *Brit.* and *For.Med.Chir.Rev.*, Jan. 1848, p.232.

66. *Op.cit.*, Note 48 above, p.727.

67. R. Fowler, 'If vitality be a force having correlations with the forces, chemical affinities, motion, heat, light, electricity, magnetism, gravity, so ably shown by Prof. Grove to be modifications of one and the same force', *Rep.Brit.Ass.Advmt Sci.*, 1850, pp.77-78 in transactions of the sections.

68. *Op.cit.*Note 48 above, p.746.

69. D.M. Needham, *Machina carnis, the biochemistry of muscular contraction in its historical development*, Cambridge, Cambridge University Press, 1971, p.595.

70. *Op.cit.*, Note 48 above, pp.746-747.

71. L.R. Wheeler, *Vitalism: its history and validity*, London, H.F. & G. Witherby Ltd., 1939, p.47.

72. See T.S. Hall, *op.cit.*, Note 22 above, especially pp.66-148, 219-287.

73. J.B. Boussingault, *Rural economy, in its relations with chemistry, physics and meteorology*, translated by G. Law, London, H. Baillière, 1845.

74. *Op.cit.*, Note 48 above, p.751.

75. *Ibid.*, pp.751-752.

76. W.F. Edwards, *On the influence of physical agents on life*, translated from the French by Drs. Hodgkin and Fisher, London, S. Highley, 1832.

77. J.C. Schiödte, *Specimen Faunae Subterraneae, bidrag til den underjordiske fauna*, Kjöbenhavn, 1849, cited in Carpenter, *op.cit.*, Note 48 above, p.754.

78. *Op.cit.*, Note 48 above, pp.755-756.

79. *Ibid.*, p.756.

80. *Ibid.*, p.757, footnote.

81. W.B. Carpenter, *Principles of physiology, general and comparative*, London, J. Churchill, 1851.

82. *Ibid.*, pp.viii-ix.

83. *Ibid.*, p.85.

84. J. W. Draper, *A treatise on the forces which produce the organization of plants*, New York, Harper & Bros., 1845, cited in *ibid.*, p.46.

85. *Ibid.*, pp.187-188.

86. *Ibid.*, p.764.

87. W.B. Carpenter, 'On the phasis of force', *National review*, April 1857, cited in J.E. Carpenter (editor), *op.cit.*, Note 2 above, pp.173-184.

88. *Ibid.*, p.180.

89. *Ibid.*, p.181.

90. *Ibid.*, pp.182-183.

91. W.B. Carpenter, *Principles of human physiology*, 6th edition, London, J. Churchill, 1846.

92. *Ibid.*, pp.1-16.

93. *Ibid.*, p.15, footnote.

94. *Ibid.*, p.1.

95. *Ibid.*, p.1.

96. *Ibid.*, p.14.

97. *Ibid.*, p.14, footnote.

98. *Ibid.*, p.16.

99. W.B. Carpenter, 'Man the interpreter of Nature', *Presidential address to the 42nd meeting of the British Association for the advancement of Science, Brighton, 1872*, reprinted from the Association's Report, London, J. Murray, 1873, cited in J.E. Carpenter, *Op.cit.*, Note 2 above, pp.185-210.

100. B. Willey, *Nineteenth century studies*, London, Chatto & Windus, 1949.

101. *Op.cit.*, Note 99 above, p.207.

102. *Ibid.*, pp.208-209.

103. *Ibid.*, p.210.

104. *Op.cit.*, Note 2 above, pp.40 & ff.

105. Y. Elkana, *The discovery of the conservation of energy*, London, Hutchinson Educational, 1974.

106. Cited in *op.cit.*, Note 2 above, p.82.

CHAPTER 8. John William Draper (1811-1882).

Draper studied chemistry at University College, London, and on emigrating to the United States took up the study of medicine at the University of Pennsylvania, graduating in 1836. He had already contributed papers on physiology to the *American Journal of Medical Sciences*, and in 1836 was appointed professor of chemistry and physiology at Hampden Sidney College, Virginia; in 1839 he accepted the chair of chemistry and physiology at New York.

Draper became one of the most distinguished scientists in America, contributing original work and many papers on molecular physics, physiology and chemistry; his principal papers were on light and heat; indeed he seems to have been the first to produce daguerreotype portraits (1839), to take photographs of the moon (1840) and to combine the camera with a microscope, using his micro-photographs to illustrate his physiology lectures. In 1875 the American Academy of Arts and Sciences awarded him the Rumford Medal for his researches on radiant energy. Perhaps the greatest of his physiological achievements, in his own opinion, was to apply to nations the same laws of growth and development as he found in his physiological research,[1] presenting his conclusions in his *History of the intellectual development of Europe* (1862),[2] which has been translated widely. To historians of science he is probably best known for his *History of the conflict between science and religion* (1874).[3] Out of his many writings, (for instance, he communicated 54 papers to the Royal Society of London), only his two physiological treatises will be discussed below.

These physiological works reveal the primary position of power in his science. In one especially obvious way, his physiology and dynamics differed from those of the physiologists discussed already - namely, in his knowledge and faith in chemistry. He therefore tackled his physiology very much as Liebig was doing at approximately the same time, but it is interesting to note that although he must have read Liebig's *Animal Chemistry*, he did not cite him even once in the two treatises about to be discussed.

These treatises had several features that are worth preliminary mention: we find that Draper had read widely on the question of power and was eager to discuss the experimental studies of many eminent European philosophers; the chemist Antoine César Becquerel (1788-1878), Charles Daubeny (1795-1867), Michael Faraday, William and John Herschel, Karl Jacobi (1804-1851), Macedonio Melloni (1798-1854) and Alessandro Volta were often cited.[4] Among the several British physiologists he cited were Peter Roget and William Carpenter.[5] He hardly ever cited pre-19th century natural philosophers or the British metaphysicians, for he seemed to be more concerned with ideas based on solid experimentation. However, despite his obvious skill as an experimental scientist and his open allegiance to Positivism,[6] there were strains of quite lyrical natural philosophy and natural theology in his writings, especially when he extolled the Sun as the great provider of power for the living world; such passages remind one of the complex natural philosophies of men like James Hutton and Humphrey Davy.

Draper protrayed power on a much more expansive canvas than had any of the physiologists so far discussed. In this respect, he was in the same class as Davy and Liebig; all three of them discussed the broad, social context of power, in addition to its purely scientific content; they seemed to be endorsing Bacon's vision that knowledge would be power, and some of Draper's writings seem to have had Bacon clearly in mind.

Draper was a fully committed mechanicist. It was therefore inevitable, given the extent to which the physical sciences had progressed and his own skill in chemical experimentation, that he would investigate somewhat quantitatively the connexions between inorganic and organic powers. In such manner he investigated topics that were at the very core of animal and plant physiology, for instance, the circulation of the blood in animals and of the sap in plants. His explanation of the powers that maintain these circulations bore some resemblance to the ideas of the physiologist William Alison.[7]

Two of Draper's publications will be discussed; they were both major treatises and together discuss all his important scientific work; they were read by leading contemporary scientists both in America and Britain, for instance, by William Carpenter, who frequently cited them.

In 1844 Draper published *A treatise on the forces which produce the organization of plants*.[8] In the preface he mentioned that he had been applying detailed chemical analysis to vegetable physiology since 1833 and that his two main physiological pursuits were chemistry and the study of the powers of Nature. In the preface to the second edition (1845) he declared that the popularity of the first edition had shown the validity of his approach and that

> "the agency of the imponderables in determining the production of chemical results, and more especially those connected with the growth of plants, is beginning to obtain the attention of philosophers, and doubtless before long will constitute a separate science".[9]

Both as a chemist and as an investigator of physiological dynamics Draper was fascinated by the continuous process of death and rebirth in the living system; as a chemist, he discussed the carbon cycle (as we would call it nowadays); from the point of view of power, be analogised the living system with inorganic ones, the main points of these analogies being that power could never be created *ex nihilo* and that all its manifestations are correlatable with one another.

> "If an electric current is to be passed along the wire of a voltaic battery, and is required to evolve a certain amount of light or heat, or to produce a certain amount of electro-magnetic motion, a fixed amount of zinc must be consumed. If a steam engine has a given quantity of work to perform, a given quantity of coal must be burned. So also in animal systems, the production of motion can only be effected by the consumption of the parts of the animal machine".[10]

The great originator of all power on the earth is the Sun: on this theme he became quite lyrical, the Sun being praised as the giver of Life and Civilization by virtue of the power of his light:

> "How is it also that wild birds and beasts conform in their habits to the progress of the seasons? ... Those migrations of fishes that take place at given seasons, and which are even connected with the wealth and well-being of nations, are determined by the occurrence of certain epochs.... If in any of these cases we pass from fact to fact, we uniformly come at last to the same conclusion, that all these incidents are directed by astronomical events; that THE SUN not only determines periods of awakening and sleep, of growth and decay, but there is also committed to him a control and regulation over all the movements of animated beings on the face of the globe.... This physical agent /the light and heat of the sun/, thus eternally but invisibly continuing its operation, produces a thousand events in which its agency is only remotely traced; nor are those influences limited to merely physical results; they stand in connexion with the progress of society and the evolution of mind. A full development of the reasoning faculty can only take place where physical circumstances conspire. It is to the climate of England and France that the human race is indebted for the intellect of Newton and Laplace".[11]

Thus, the power of the sun eventually becomes manifest in the power of the mind; that in turn produces the power of civilization, for advances in knowledge are advances in power. Draper believed that the civilized man of the mid-19th century was an utterly different being from the man who had lived a thousand years ago, and that the conditions determining man's position had changed totally: for modern man, the position both of empires and individuals is fixed by the possession of knowledge - knowledge which is incessantly advancing. Wherever intelligence has been given, there is a requirement to join in the advancing march. The Indian stands still, and the penalty is death.[12] At the very end of the first chapter Draper described the general purport of his treatise; it was designed to be a treatise on the paramountcy of power, and vegetable physiology was merely a suitable illustration of it:

> "... that whatever motion is accomplished, or whatever change is brought about, there is a consumption of material or an expenditure of force; that as the surface of the earth is continually remodelled by physical agents, so are the vicissitudes through which organized forms pass determined by physical powers, and bring about physical ends.... If, moreover, our thoughts are directed to the relations which exist between climates and the character of races, the distribution of vegetables and animals; if we observe the antagonization of these great classes in the result of their vital processes, their position as respects the atmosphere, the control which astronomical events possess over everything, ... we surely shall have but little difficulty in understanding that, as in the inorganic world, so also in the world of organization, those all-pervading forces which natural philosophers and chemists recognize are constantly employed".[13]

In chapter two he discussed his *cause célèbre* - the circulation of fluids in animals and plants; he asserted that the basic force maintaining all such circulations was chemical affinity, though this actually manifested itself as capillary attraction within the circulatory vessels. Thus, if two liquids communicated with one another in a capillary tube, or in a porous or parenchymatous structure, and had for that tube or structure different chemical affinities, movement would ensue; the liquid with the most energetic affinity would move with the greatest velocity and might even drive the other fluid entirely before it; all this was due to 'common capillary attraction', which in its turn was due to electric excitement.[14] He believed this was both new and important in its wide applicability; for instance, "... even in MAN, the circulation of the blood is caused by the oxydating action of that liquid on the solid structures with which it is brought in contact".[15] Ultimately, however, the primary power of all was still the light of the sun.

In chapter three Draper argued that, since the primary cause of the blood's circulation was chemical affinity - the affinities between the oxygen of the arterial blood and the carbon and hydrogen of the body's tissues - the heart was only a regulator of the circulation; and as in a steam engine, where a change in its motion is eventually impressed upon the governor which adjusts itself accordingly, so would the heart act as a governor and accomodate itself to the changes in the blood system.[16] Essentially then, the prime mover of the circulation was supposed to be respiration - not the traditional notion of respiration as occurring primarily in the lungs, but the new notion of tissue-located respiration, namely, the interaction between oxygen and other elements within the extra-cardial parts of the body. For this scheme Draper had a certain amount of empirical evidence. That his scheme turned out to be correct does not concern us here; what does concern us is that he reached that idea by considering the body's functions not only as a mechanicist but also as a chemist:

> "To the mind of a chemist, the relation which exists between arterial and venous blood and the soft tissues of the animal body may be very forcibly impressed by these considerations".[17]

Having discussed the dependence of the body's powers on the powers of inorganic Nature in the first three chapters, the remaining chapters reviewed experiments and hypotheses on the question of whether such powers might be reducible to one fundamental type. Could all forces be correlated? Draper reviewed studies done by the two Herschels, Joseph Fraunhofer (1787-1826) and Macedonio Melloni on the questions whether the light and heat from the sun are essentially identical, or at least derived from a common agent, and whether they are similarly correlated with the chemical powers that seem to result from them. Some ten years earlier, Melloni had concluded that light and heat were two physically independent principles, but recently he had changed his mind, opining that "light is only a series of calorific indications sensible to the organ of sight, or vice versa, that radiations of dark heat are true, invisible radiations of light".[18] Draper admired Melloni's experimentation but disagreed with his conclusion, for he did not think the experimental evidence yet adequate. Just as in chemical analysis, where chemists considered it absurd that Nature should possess 40-50 different metallic elements when their differences were often slight, but that until more evidence became available they could not "go back to the alchemical doctrine"[19] of only a few fundamental elements, so too with the enumeration of the imponderables; Draper believed that eventually the essential identity or correlation of these agents would be proven:

> "That some splendid generalization will hereafter unite all these imponderable principles, we have repeatedly said; but there are very many facts now known which none of the views hitherto brought forward can embrace. Under these circumstances, it would seem that the proper course to pursue is to regard each one of these agents as physically distinct".[20]

As we shall see, such a belief in the eventual discovery of a generalizing principle for force, moderated by an awareness of the insufficiency of experimental evidence thitherto available, was shared by contemporaneous natural philosophers and physiologists in Germany; we have already seen the similar current in Britain. Of course, this was no coincidence for, as Rothschuh asserts,[21] the development of physiology in the United States during the entire 18th and first half of the 19th century followed the European antecedents. Indeed, Rothschuh suggests that American physiology remained in its first, pre-experimental phase, as a field for study outside the laboratory, until c1860; of course, there were exceptions, for instance William Beaumont (1785-1853) and Robley Dunglinson (1798-1869) who did pioneer research on digestion; and he gives a paragraph to Draper.[22] It is well to bear Rothschuh's point about the derivativeness of American work in mind when reading Draper for, although he cited physicists and chemists readily, he does not seem to me to have cited as many physiologists - particularly Germans - as he might have. Perhaps he regarded their work as too tainted with the spirit of *Naturphilosophie* to deserve citation.

An interesting point in his discussion on power was an argument for correlation of forces that had been used by the chemist, Antoine Becquerel, and which reminds us of the basic idea behind Johannes Müller's specific sensations theory:[23]

> "This brings me to offer some remarks on the opinion expressed by M. Becquerel, that the phenomena now under discussion are due to the qualities of the receiving surfaces, and not to agents intrinsically different, coexisting in the solar beam. That the same *beam of light*, falling on sulphuret of lime, causes it to phosphoresce; on chloride of silver, blackens it; on the retina, gives rise to the phenomena of vision and colour; on a piece of black cloth, causes it to become warm. This opinion seems to be surrounded with insurmountable difficulties and, if admitted, would disturb some of the best established truths of science".[24]

Müller's theory was not actually mentioned, and since it was not available in English until Baly's translation of the *Handbuch der Physiologie* in 1838 and 1840, it is not improbable that Draper did not know of his ideas. However, it is worth noting how the consideration of sense-perceptions could lead onto the question of the correlation of powers; this is precisely what happened with Müller himself and with his pupil, Hermann Helmholtz. (See chapters 13 and 16 of this thesis).

Draper thought that, in the absence of contrary evidence, he might propose at least four different powers in sunlight:- 1) Visible light; 2) The principle of radiant heat; 3) A principle of chemical force, which seemed to be intimately associated with light, yet differed from it in wanting the power of affecting the human eye. From its intimate association with light, Draper suggested for this principle the name of TITHONIC RAYS, in allusion to the fable of Tithonus and Aurora;[25] and 4) A principle of phosphorescence. In his experimental study of photosynthesis, Draper believed that he had managed to separate these four powers;[26] for instance, he had shown that the decomposition of carbonic acid in leaves was due to that part of the sun's spectrum where there was maximum visible illumination in the yellow region, and that therefore this was not due to the yellow calorific or the yellow tithonic power, but only to the yellow *light* power.[27]

One overall impression one gets from Draper's treatise is that his investigation of photosynthesis since the 1830s had led him into the general topic of power, which had since become his principal interest. Consequently, the problem of unravelling plant metabolism was a problem of dynamics as well as of detailed chemistry; and although he nowhere quoted Liebig, this was precisely the same approach as Liebig's. What most impressed Draper was the immensity of the power in the sun's rays; the question of what happens to all that power inevitably forced itself upon him (as it had upon Hutton and Davy and was to upon von Reichenbach, Liebig and Helmholtz).[28] His solution was that it is metamorphosed into the various terrestrial types of power and that power is therefore neither destroyed nor created. Plant metabolism had therefore led him onto a question of cosmic significance:

> "In what manner, then, does this light act? ... What are the corresponding and contemporaneous changes which happen to the light? Action and reaction are always equal, and if a given beam can produce a result which demands the most energetic chemical force, it is reasonable to suppose that in doing so, it undergoes itself a change.
>
> These considerations show us that the question in what manner yellow light acts in controlling the function of digestion in plants, is not only exceedingly interesting in a physiological point of view, but also that it involves the whole theory of radiant matter, whether it be of light, heat or tithonicity,in producing chemical change.
>
> From the combustion of small quantities of carbon, we see, in improved steam engines how great an amount of force can be originated, and by the oxydation of a few grains of zinc in voltaic batteries, what surprising chemical results arise. From those more ordinary cases of changes accomplished by the action of light, which appear to be feeble and slowly produced, we should form the most erroneous opinions of the force of the sun rays. General considerations might lead us to know that the principle which has in charge the keeping up of the constitution of the atmosphere, and regulating the vital functions of plants, is of great intensity. Thus, I have found that the rays which are emitted from a common wax candle are superior in chemical force to the current which is evolved by a cell of Grove's battery, the most powerful of voltaic combinations known, for they could effect the recomposition of muriatic acid much faster than the battery could decompose it,

> and yet that battery was found competent to maintain a platina wire white hot, and if the views of Dr. Faraday are correct, was evolving more electricity than is developed by any thunderstorm. If this is the case with a candle, what, then, shall we say of the brilliant rays of the sun, which impinge on the earth on all sides?"[29]

Coming now to his *Human physiology, statical and dynamical* (1856), we find reiterated therein all the salient ideas of the earlier treatise, some developed in the light of more recent scientific studies. Once again, the correlation and conservation of powers were championed and occupied an important place in his physiology. By now, of course, these principles had become fairly accepted in western science, for it was nine years since Helmholtz had read his classic paper. Nonetheless, it is worthwhile to discuss what these principles meant to Draper as a physiologist, and we may still regard his use of them as largely original, for he had been developing his own line of thought since the 1830s.

His preface to *Human physiology* was especially noteworthy, for its allegiance to Auguste Comte (1798-1857) and his philosophy of 'positive science';[30] no longer were the old metaphysical philosophies of the "English, Scotch, French and German schools" relevant;[31] Locke, Hume, Reid, Stewart and others were of no use to Draper's science. Physiology was that branch of positive science which Draper most wanted to propagate:

> "How many advantages would arise if the elements of this science were made a part of general education in America!"

And "That a great revolution is impending in the practice of medicine, no one who is at all observant of the progress of science can doubt. The great physicians of the future will be the great physiologists".[32]

The first part of the treatise - 'statical physiology' - was less explicit on the relations of power than the second part, but it did contain some germane discussions; for instance, Draper had become more assured that the primary causes of the blood's circulation were the chemical affinities entailed in respiration; respiration was now located unhesitatingly in the body's tissues,[33] and there was therefore a grand correlation between 1) the chemical powers of oxygen and the components of the body, 2) the movement of the blood, 3) animal heat and 4) animal metabolism.[34] Vital functions were easily accounted for by employing these powers; for instance, the five senses could be explained by the metamorphoses of external forces into their physiological equivalents. Thus, in the mechanism of the eye, the first impression on the retina might be heat,

> "... and though in this manner, the origin of the action which has been set up is calorific and therefore physical, it immediately becomes converted into a physiological equivalent in the metamorphosis and destruction of a nervous tissue".[35]

But "It now remains to add that this is only one manner of looking at the thing. According as our hypothesis of the nature of light, of its relations to heat, and of its manner of establishing chemical changes may be, the special explanations we give of the functions of the eye will differ; yet there is such a relationship among these hypotheses that we can, without any

> difficulty, convert an explanation derived from one into an explanation derived from another. It really comes to little more than a translation of phraseology".[36]

This explanation was fully reconcilable with Müller's theory of sense-perceptions, though again that was not cited. However, he did cite Müller's study of the vocal organs,[37] and we may believe that by that time (1856), he had become acquainted with most, even all, of Müller's principal physiological researches.

The second part of Draper's *Human physiology*, on 'dynamical physiology', was bolder in its use of power. This twofold division, into statical and dynamical, was not arbitrary but was, he claimed, modelled on Galileo's division of mechanics into the same categories.[38] (We saw in Chapter 1 of this thesis that Hume had divided his analysis of mind into statics and dynamics, apparently modelling himself on Newton. Draper might well have known of this, but he did not admit it.) This twofold division was extremely useful to Draper, for his physical science, his physiological science, his anthropology and his theology could all be served by a single paradigm - power. Even chemistry could be explained by dynamical laws, one of which was the conservation of power:

> "If, then, our earth does not possess within herself the power of sustaining the varied forms of vegetable life, but borrows it from an extraneous source; if light, in producing these effects, never undergoes destruction, but only modifies its state - for neither force nor matter can be annihilated, though they can be changed - what shall we say of

the plastic power which we have thus assumed
to reside in the germ?"[39]

Ultimately, the powers in the germ, as in all organisms, were seen to be derived from the sun. Draper claimed that to the physiologist the sun was no less sublime and powerful than it had traditionally been to the astronomer. All heat and light came from the sun; and Draper meant much more than a metaphor in saying that: "The sunbeam is the finger of God".[40] Often, Draper sounds quite lyrical, even religious, in his scientific writing, and nowhere more so than in his contemplation of the powers of the world. In this blending of 'positive science' with religious feeling he was, of course, simply following the example of Comte. Like his master, Draper considered nothing to be outside the aegis of his positivism. In particular, he considered nothing to be outside the aegis of his philosophy of power. Not surprisingly, therefore, he used that philosophy even to defend the immortality of man's soul against the atheists:

> "If there is a point in natural philosophy which may be regarded as finally settled, it is the imperishability of the chemical elements and the everlasting duration of force. With the system of Nature existing as it is, we can not admit that an atom of any kind can ever be destroyed; and a like assertion may be made of force. Heat may give rise to motion, motion to electricity, electricity to heat: one kind of force may be converted into another, there being a perfect correlation or quality of of substitution among them. The quantity of power is now the same as it ever was. Its variations are analogous to the apparent transmutations of ponderable material. They are mere metamorphoses.

> ... and if thus neither matter nor force can die, it would be a great anomaly if the principle of conscious identity were capable of annihilation. Like them, it may be capable of modification or change, and like them it is not capable of loss of existence....
>
> Perhaps,in some age hereafter physiology will find herself sufficiently advanced to offer her opinion on this profound topic, for I cannot think that GOD has left us without a witness in this matter, even in the structure and development of the body itself".[41]

<u>Summary</u>

The topic of power, especially the correlation of its various forms and its net conservation in the world, occupied pride of place in Draper's natural philosophy and physiology. It determined the pattern of his thoughts and of his experiments, so that he became totally absorbed by it. He was, however, no mere metaphysical enthusiast for power; he was a skil~~l~~ful experimentalist, and many portions of his treatises were given to detailed discussion of his own and others' experimental researches. He undoubtedly tried to test hypotheses by experiments, which was probably why he could not admit to the correlation of powers in his first treatise, but did expound it in his *Human physiology*, for by then sufficient evidence had accumulated to overcome his wariness. In his approach to this problem he reminds one of von Reichenbach in Austria, who was investigating the relations of forces and had coined a new force, the 'Od', at about the same time as Draper was working. (Von Reichenbach is discussed in Chapter 15 of this thesis). Neither cited each other, yet both were working in the same direction and were to have their discoveries - of tithonic rays and the Od - ignored.

I have not been able to assess the influence that Draper exerted on the study of dynamics. He was cited not infrequently in Britain - for instance, by Carpenter - and was probably cited quite often in America; into that I have not researched. However, what does emerge from this brief study is that the question of force occupied at least one eminent American in the mid-19th century and was investigated thoroughly by him before he would commit himself to any all-encompassing hypothesis. Of Draper's competence in experimentation there can be no doubt, for he was a widely acknowledged pioneer in photography and microphotography, and his studies on tithonicity must have commanded attention, if not assent.

NOTES TO CHAPTER 8.

1. J.W. Draper, *Human physiology, statical and dynamical, or the conditions and course of the life of man,* London, Sampson Low, 1856. See the ultimate chapter 'On social mechanics', esp. pp.602-603.

2. J.W. Draper, *History of the intellectual development of Europe*, New York, Harper, 1862.

3. J.W. Draper, *History of the conflict between science and religion*, New York, D. Appleton & Co., 1875. Recently reissued by Gregg International, New York, 1970. There is a succinct and useful discussion of this book in C.A. Russell, *The conflict thesis and cosmology*, Unit 1, Milton Keynes, The Open University Press, 1974.

4. J.W. Draper, *A treatise on the forces which produce the organization of plants*, 2nd edition, New York, Harper and Row, 1845; see esp. chapters 4, 5, 7 and 11.

5. *Op.cit.*, Note 1 above, pp.i-iv.

6. *Ibid.*, p.v.

7. *Op.cit.*, Note 4 above, chapters 2 and 3 especially.

8. *Ibid.*

9. *Ibid.*, this quotation is unpaginated.

10. *Ibid.*, p.6.

11. *Ibid.*, p.12.

12. *Ibid.*, p.8.

13. *Ibid.*, p.14.

14. *Ibid.*, p.29.

15. *Ibid.*, p.29.

16. *Ibid.*, p.40.

17. *Ibid.*, pp.35-36.

18. *Ibid.*, p.56. Draper mentions that Melloni's new ideas first appeared in *Comptes Rendues*, Jan., 1844.

19. *Ibid.*, p.70.

20. *Ibid.*, p.57.

21. K.E. Rothschuh, *History of physiology*, trans. by G.B. Risse, New York, Robert E. Krieger, 1973, p.187.

22. *Ibid.*, pp.188-189.

23. See my Chapter 13. In 1827, the physiologist Jöhannes Müller showed that the differences among our sense-perceptions do not depend on differences in the external, physical agents that impinge upon us, but that they depend upon differences in the nature of the sensory nerve terminals themselves. Thus, a blow on one's arm would cause pain, whereas one on one's eye would cause colours to be seen.

24. *Op.cit.*, Note 4 above, p.70.

25. *Ibid.*, pp.68-71.

26. *Ibid.*, chapter xi. He nowhere uses the word 'Photo-synthesis'; his phrase is 'digestion of plants'.

27. *Ibid.*, pp.68ff.

28. See my chapters 14, 15 and 16.

29. *Op.cit.*, Note 4 above, p.70.

30. There is a very readable discussion of Comte in Basil Willey, *Nineteenth century studies, Coleridge to Matthew Arnold*, Harmondsworth, Pelican Books, 1973, esp. pp.197-213. Willey discusses the reaction of British thought to Comte particularly well.

31. *Op.cit.*, Note 1 above, p.v.

32. *Ibid.*, p.vi.

33. *Ibid.*, p.127.

34. *Ibid.*, p.133.

35. *Ibid.*, p.393.

36. *Ibid.*, p.398.

37. *Ibid.*, chapter xviii.

38. *Ibid.*, p.455.

39. *Ibid.*, p.460.

40. *Ibid.*, p.466.

41. *Ibid.*, pp.548-549.

Chapter 9. On power or force in the physiologies of Richard Fowler (1765-1863), George Holland (1801-1865) and Orson Squire Fowler (1809-1887), and in William Paley's natural theology. Conclusion to the preceding chapters.

This final chapter on British dynamical physiology is a mixed bag. It discusses three physiologists who contributed to the physiological application of the correlation of forces. They are discussed because, although not great physiologists, they held ideas which were fairly original and struck their contemporaries as ingenious. Moreover, men like Roget and Carpenter cited them not infrequently. They differed from each other in the character and significance of their work: Dr. Richard Fowler conducted an admirable, experimental investigation into galvanism, with the intention of testing the various hypotheses which were in the air concerning the relation of galvanic to other forces; that was published in 1793, and he was still beavering away in the 1840s and 1850s, as we shall see from papers he read to the British Association for the Advancement of Science. He was obviously an early and widely respected researcher in physiological dynamics; Johannes Müller cited him and Carpenter acknowledged the value of his work in his 1850 Royal Society paper. But, despite his promising start and doggedness, he was never especially influential.

Dr. George Calvert Holland (1801-1865) has been chosen as an example of a now-forgotten physiologist who used the concept of power to its utmost and was especially intent on using it in his choice of *materia medica.* He was in the John Hunter - William Alison tradition of focussing especially on the circulation of the blood and its powers; like Carpenter, he was interested in using dynamical physiology to prove the folly of sexual overindulgence, thus giving scientific support to society's traditional moral attitude. Despite the idiosyncratic nature of his ideas, William Carpenter referred approvingly to his work.

The third physiologist was Orson Squire Fowler (1809-1887), a well known American phrenologist. He also gave much attention to proving scientifically the folly of sexual excesses; moreover, no physiological or pathological phenomenon was beyond explanation by his dynamical principles.

The ultimate section will discuss the place of power in William Paley's immensely popular *Natural theology*, a treatise that no historian of 19th century ideas in Britain can afford to ignore. Paley runs in the Locke - Reid - Stewart tradition of philosophy of power, and it is scarcely an exaggeration to say that every natural philosopher reared in Britain during the first half of the century read him.

Richard Fowler

In 1793, *Richard Fowler*, a young Edinburgh physician, published a slim treatise called *Experiments and observations relative to the influence lately discovered by M. Galvani, and commonly called animal electricity.*[1] His aim was to investigate whether Galvani's principle or force had any intimate relation with other principles, such as ordinary electricity; Eusebio Valli (1755-1816), Galvani's nephew, had contended that there was a relation. Fowler's book described many experiments, some simple, others intricate, that he and other researchers had done, and even today one must be impressed by its thoroughness and total reliance on experimental data; it greatly impressed his contemporaries in Britain and replaced Valli's work as the most authoritative analysis of galvanism.

Having concluded in sections 1 and 2 of his book that galvanism had no relation to electricity or magnetism, he asked "What are the relations between the influence discovered by Galvani, and the muscles, the nervous and the vascular systems, of animals?"[2] After reviewing all available evidence he concluded regretfully that no essential relations had yet been discovered, although it seemed as if the blood-system might be involved in the source of galvanic power. One of his finest series of experiments was on sight; it is noteworthy for its thoroughness, for the questions it raised on the relations between inorganic and organic powers, and because this field was to become so important in the next few decades, especially in the hands of Johannes Müller. Fowler began his experimental investigation of sight because the eyes are affected easily by metallic combinations. Laying a piece of tin-foil on the tip of his tongue, and the end of a piece of silver in the inner canthus of his eyeball, and bringing the metals into contact, a pale flash of light spread over his eye. On darkening the room, the flash was more vivid; if zinc and gold were used the flash was yet brighter. Recollecting that nerve filaments pass from the ophthalmic ganglion, through the eye's sclerotic coat, to the choroid coat and uvea, and that the ganglion is formed partly from the nasal branch of the fifth pair of nerves, he passed a piece of silver up into his nasal passage as far as possible and brought it into contact with a piece of zinc placed on his tongue; as expected, he experienced a flash of light.[3]

Although Fowler did not offer any theory about a connexion between galvanism, light, taste and chemical affinity, such as could account for the above experiments, he probably thought deeply along that line in the next few decades, for in 1849 he delivered his British Association paper: 'If vitality be a force having correlations with the forces, chemical affinities, motion, heat, light, electricity, magnetism, gravity, so ably shown by Professor Grove to be modifications of one and the same force'.[4] From the abstract of it in the official *Report* of the meeting we learn that, having argued for the interconvertibility of the physical forces, he asserted that vitality was equally freely interconvertible with them. One of the main differences between physical and organic forces, he said, lay in the mechanisms of their interconversions, particularly the coils that experimenters like Faraday had been using with such success. (Faraday had constructed coils of wire around magnets and pieces of iron and with such apparatus had discovered electromagnetic induction in 1831). Fowler employed such coils as analogues to imagine how forces,previously latent, might be made apparent to human senses: for instance, the temperature change to which a newly born infant is subjected excites the motion needed for inspiration of air; this gives rise to chemical reaction between oxygen and carbon, this again to animal heat, from which animal electricity might be obtained; from such electricity, "by an appropriate coil", magnetism is obtainable. Unlike mere physical apparatus, however, vitality is "the artist of its own coils"; no other force is known which could produce a piece of mechanism to reproduce itself. Finally, mind and vitality are correlated with each other and with the physical forces.[5]

Clearly, Fowler was thinking along the same physiological lines as Carpenter, and the full enunciation of the correlation of forces by Grove gave him the opportunity and incentive to present his own ideas more boldly, with the assurance that he would be heard with some respect by his scientific colleagues.

Fowler does not seem to have influenced any of the other contributors to the correlation and conservation principles, and apart from his early treatise on galvanism, he was not widely quoted. However, the persistence of his interest in the relations of forces throughout his life, and that this theme was central to his own view of Nature, can be seen from the fact that he was still discussing its physiological implications in a paper he read to the British Association in 1859, at the age of 94. There, he explored the question that Carpenter was to raise in his Royal Institution lectures later that year, namely, the dynamical relations between mind and matter. The main purport of this paper, entitled 'A second physiological attempt to unravel some of the perplexities of the Berkleyan Hypothesis',[6] was to disprove Berkeley's assertion that a conception or image has no existence except while it is being perceived. Locke had dealt with this theme of image formation earlier. Fowler agreed with him but wanted to give a thoroughly dynamical explication of the persistence of images in the human mind:

> "Now the bridge to connect mind with what is external to mind will be found, I think, in the pre-established affinities of the forces with which phenomena are composed, and the mind which conceives them. Such affinities consitute the pre-established harmony suggested by Leibniz.
>
> All chemical affinities are of this kind; all sensational, all intellectual, all associations of ideas, the affinities of force for each other, as magnetism for iron (see Ampère)".[7]

> "The vitality of sap in trees is so modified by the graft coil through which it passes, as to result in varieties of fruit corresponding with the graft. The motion by which a ship moves is modified by the adjustment of the sails, the rudder, paddles and screw. Now, the law of these forces requires investigation, and is clearly (as Turgot and Dugald Stewart asserted) independent of the mind, and external to it. May it, then, not be asserted, as affirmed, that the forces are the bridges by which the mind passes to and from the phenomena which it perceives?"

> "I am afraid I may not have been sufficiently explicit as to the means by which the severance between matter and mind may be bridged over by an affinity, or a force; but I consider that, in addition to the 7 physical forces, of which Mr. Grove has so ingeniously proved the correlations, mind and vitality are equally forces, as I have attempted to prove in former papers, and that these - mind and vitality - have such correlations with the physical forces as to form the communication which bridges over the apparent severance between mind and matter".[8]

Richard Fowler must have been the grand old man of British scientific society in the late 1850s and early 1860s, renowned not for any seminal scientific discovery but rather for his magnificent staying-power and loyally supporting the annual meetings of the British Association. His scientific acquaintances must also have been impressed by his persistence in exploring the relations among the powers of Nature, and it is just possible that there was a closer connexion between his work and Carpenter's and Roget's, than I have yet discovered. Certainly, he had much in common with both Carpenter and Roget, for they were all fellows of the Royal Society, members of the British Association and physiologists with more than passing interests in the latest researches on forces. All three were graduates of Edinburgh.[9] Perhaps the most significant difference between Fowler and the other two was that he never lived in and rarely visited London, whereas they spent most of their careers there; that would account for Carpenter's apparent unawareness of his work until 1850.

George Calvert Holland was known to British physiologists for two quite impressive treatises - his *Experimental inquiry into the laws of organic and animal life* (1829),[10] and his *Inquiry into the principles and practice of medicine, founded on original physiological investigations*, Vol.1, (1834), Vol.2, (1838).[11] Since the latter treatise contained all that was important in the former, and much more, it alone will be discussed in detail here. The aim of both works was to discover some of the general laws which regulate the well-being of the vital powers. In the earlier work, he had concluded that the two essential centres of power in the animal organism are the circulation of the blood and the respiration,[12] that the energies of all other vital functions depend upon the power supplied to them by these two, and that ailments are mostly due to essentially dynamical derangements in the blood's circulation.

These conclusions were developed in the later treatise. There, for instance, he criticized Cullen's doctrine of spasms, his *vis medicatrix naturae* and his notions on the nervous system, because Cullen's powers were not fundamental enough; if only he had studied the powers connected with the blood, he would have been able to explain spasm etc., more simply.[13] Holland's main criticism of other physiological systems was that they had failed to explain how the properties and circulation of the blood were modified by external and internal forces, how the blood system was the main co-ordinating mechanism in the animal, and the laws regulating the generation of animal heat.[14] He envisaged the animal system as a store of power, the principal source of which was the chemical energy of food and oxygen. Although he never explicitly discussed the possibility of a real correlation between vitality and inorganic forces, and cannot be said to have contributed directly to the principles of correlation and conservation, he clearly believed that the living organism could not create its own power and that exhaustion of its power was the key idea in accounting for, and treating, ailments;[15] excessive sexual activity was one of the most dangerous ways to exhaust the animal frame, his views on which were very similar to Carpenter's. Thus,

> "Among other important and frequent causes of constitutional exhaustion, associated with nervous irritability, may be enumerated the unrestricted indulgence of the animal desire, or even the ordinary gratification of it in feeble habits of body".[16]

> "The gratification of the sensual passion is more frequently a cause of disease than is generally imagined. My own observation, which has been particularly directed to the investigation of this subject, has furnished me with many instances strongly illustrative of the injurious effects of such indulgence on the constitution. Consumption is often produced by it ...."[17]

> "The undue indulgence of the amative propensity produces, of course, different effects in different constitutions: in some consumption, in others various diseases of the nervous system, and not unfrequently scrofulous affections, attacking the spine, mesenteric and other glands".[18]

As with John Brown and Benjamin Rush, Holland's dynamical physiology did have the beneficial effect of proclaiming that the whole patient - his surroundings, state of mind and body - must be cared for whenever he suffers any ailment, be it psychological or physical. This can be seen especially in Volume 2 of his *Inquiry*, 'On cholera', where he offered a new view of the disease, namely that it was caused by an agent or external power which, acting upon an already weakened body, caused a "depression of the circulatory powers, and a consequent internal determination of the blood". The initial weakness could be caused by climate, or even a state of mental depression; since mental power seemed to be only one form of vital power, he believed too that the mental state was often the occasion of cholera, dysentery and fevers.[19]

Most of the remedies he proposed for cholera were dynamical, aimed at inducing the whole animal economy to recover 'its natural energies'.[20] The most important remedies were: First, a calm and cheerful state of mind. Second, nourishing and stimulating diet. Third, warm clothing and habits of temperance.[21] Others were galvanism, especially in states of collapse; and muriate of soda, for this would aid the oxygenation of the blood, whereby the blood could impart more power to the body's tissues and more readily maintain the animal heat.[22]

As with other physiologists discussed in this dissertation, Holland's ideas on the physiological rôle of power were often vague, and are difficult for an historian to evaluate. Certainly, he was far below Peter Roget and William Carpenter, with regard to both the thoroughness and experimental reliance of his physiology, and his contributions to the energy question. Yet Holland's two treatises were regarded well by British physiologists in the second quarter of the century, and it is probable that the doctrine of correlation, especially as enunciated by Carpenter and Richard Fowler, would not have surprised him for it accorded well with his own ideas.

<u>Orson Squire Fowler</u>

In 1850 a collection[23] of papers on phrenology and physiology by Fowler was published in New York. Fowler had already written several works on diverse topics, and during the next 30 years he was to widen the scope and discursiveness of his physiological writings. According to two historians of American physiology, his books on love, marriage and physiology touched on everything from animal magnetism to phrenology, from birthmarks to the dynamical principles of health foods, from the evils of wearing corsets to composing a prayer for those addicted to 'self-abuse'.[24] Fowler's principal physiological interest was in the body's forces; health was the maintenance of a due balance and an adequate level of these forces, whilst ailments were primarily caused by exhaustion. Occasionally he seemed to write as a vitalist, though more often he used a sort of correlation theory to account for vitality in terms of physico-chemical forces, especially magnetism. Indeed, it is difficult to extract a totally consistent theoretical basis from his writings, but it is certain that he regarded the organism essentially as a storehouse of power in various forms, that every action of the body entailed an expenditure of that power, and that the forms of living power were correlated one with another and with the powers obtainable from food, air, sunlight etc.

His theory is well seen in one of the pamphlets, 'Amativeness; or evils and remedies of excessive and perverted sexuality',[25] in his 1850 book. There it appeared that he was not alone in his belief that sexual excesses could lead directly to grave maladies such as fevers, insanity and acute gastro-intestinal disorders. Of the letters that fellow American physicians had written to him, the following is typical:

"Brooklyn, Dec.19th 1840.

> ... In my own practice, I think I have seen the following results of masturbation -

> involuntary emissions, prostration of strength, paralysis of the limbs, hysteria, epilepsy, strange nervous affections, dyspepsia, hypochondria, spinal disease, pain and weakness in the back and limbs, costiveness and in fine the long and dismal array of gastric, enteric, nervous and spinal affections, that are so complicated and difficult to manage".[26]

To this Fowler added that the loss of seminal secretion is the loss of vitality itself, and that since it is a well known physiological principle that the over-taxing of one organ robs the other organs of the vital power, excessive sexual indulgence cannot fail to weaken the whole system. Well had the Book of Wisdom said "Give not thy strength to women", and he who did had to expect to be weak everywhere else.[27]

Fowler's remedies for sexually induced debility were solely dynamical ones. The first was to abstain immediately. Then,

> "galvanism and magnetism, and Sherwood's magnetic pills, have effected some surprising cures of this disease, and especially of female complaints; yet the former, being a powerful tonic, must not be used too freely. ... physiological prescriptions and preventives will generally be found to work a more effectual cure than those medicines (of which I unequivocally disapprove) used by the faculty.
>
> Avoid all stimulants and irritants. Inflammation being the chief cause of your difficulty, everything calculated to increase it is unequivocally bad. Hence abandon wholly and at once *tea, coffee, tobacco,* and all *stimulating meats and drinks*".[28]

These remedies were what one would have expected from the old anti-phlogiston theory; that theory was now obsolete, but so long as health and disease were considered to be vaguely dynamical phenomena it was still possible to believe in the anti-phlogiston remedies.

In a paper on 'Love and parentage' Fowler tackled the question of the source of an organism's powers. Animal magnetism was the basic form of that power, although electricity and galvanism were synonyms for it. Magnetism could be called the master workman or grand executive of every animal and mental function, and even "when applied to grain, roots, herbs, etc., it accelerates their growth a thousand per cent".[29] Consequently, he envisaged the organism as a type of galvanic battery generating positive and negative forces, the alternation and expenditure of which produced every motion and function in living nature. The vital system was essentially a machine in which every action entailed an appropriate expenditure of power.[30] Thus one could explain why great men often have weakly children - because they expend so much of their energy on their labours that little vitality is left over to produce an energetic seminal secretion and hence vigorous offspring. A child's innate stock of vitality depended on the power transmitted by his parents during the act of conception; and the principal aim of a child's education was simply to build up this Lamarckian-type inherited power.

> "The great fault of modern education is robbing the body to develop the mind .... Let your first labour be to give your children strong constitutions, and to lay in for them as large a supply of physical energy as possible. You may cultivate their intellects, but not so much

as to withdraw their energies from growth".[31]

Medical practice was to aim at building up the body's reservoir of power. Food and sleep were, in Fowler's opinion, the main inlets of this power, whilst every physical and mental effort expended it. Whenever the expenditure exceeded the supply, a draft occurred on the *original* stock of vitality, namely, on the constitution itself, and the ultimate effect of that was death. Consequently, "To take alcoholic stimulants is to commit suicide in proportion to the amount taken".[32]

In like vein, when considering food, his main interest was what kinds of food develop specifically the physical and mental powers of the body. In his opinion this was one of the most momentous questions facing physiology. Fowler was a vegetarian and argued that such a diet could easily supply the body's energy requirements, as well as foster a greater moral energy than a meat diet (an argument still used by some vegetarians, to wit Mr. Smith in Graham Green's novel, *The comedians*). On the nutritional adequacy of a farinaceous diet, he quoted Liebig with great admiration:

> "His *Animal chemistry*, one of the most profoundly philosophical works on this subject ever written, thus answers this question ...."[33]

It is at first sight surprising that Fowler, firmly committed to dynamical physiology and with no training in chemistry, should have admired and extensively quoted Liebig, the high priest of organic chemistry. To a great extent dynamical physiology and the new chemical approach were mutually antagonistic, since the adherents of the former could easily explain vital functions solely in terms of force or power and rarely was the need for, or even validity of, a full-scale chemical approach; conversly, chemists considered force to be philosophically dubious, and they were obtaining promising results with their purely chemical researches. This antipathy was especially exemplified by one of Fowler's fellow phrenologists in America, Charles Caldwell (1772-1853), a former army surgeon and holder of several chairs of medicine during his long and not undistinguished career, who had written a scathing attack[34] on Liebig's *Animal chemistry* in 1843. How then, could Fowler adhere to dynamical physiology in much the same way as Caldwell did, yet admit the greatness of Liebig's physiological chemistry? It seems to me that Fowler understood the character of Liebig's *Animal chemistry*, whilst Caldwell had not; for, although Liebig was primarily a chemist, he was also greatly interested in force, and his treatises on organic chemistry in its application to agriculture and physiology, in 1840 and 1842, were written from that viewpoint. Fowler most likely realized that, since he called Liebig's work "one of the most profoundly philosophical works on this subject ever written". (My Chapter 14 refers back to this point).

Fowler's ideas on bodily powers were connected with his belief in magnetism and phrenology. However, there were many physicians who held similar ideas on the debility caused by sexual overindulgence and on the essentially dynamical nature of the body, who were not phrenologists and roundly condemned animal magnetism. Perhaps the model treatise for these physicians was the *Macrobiotik; or the art of prolonging life* (first published 1797, latest edition 1975), which had been written by the Prussian physician Christoph Wilhelm Hüfeland (1762-1844). This and other works by Hufeland had been translated widely; many of the British physiologists discussed in this thesis had read him and Carpenter quoted him approvingly; Fowler must have read the *Macrobiotik*, but since it totally rejected animal magnetism he did not acknowledge it as a useful source. Hufeland was so widely read, however, both in German and English, and was so committed to the doctrine of dynamical physiology, that he will have a chapter of his own later.

A comment on phrenology

So far in this thesis, several passing comments have been made to phrenology, but I have not attempted to evaluate any possible relevance that topic might have to my thesis. I am fairly sure that such a study would yield considerable relevant material and that several chapters might be written on, for instance, the connexion between the growth of dynamical physiology and phrenology. However, that task would have taken up more time than I could afford and will have to wait awhile. From my thesis, several points already suggest themselves for a start into phrenology. For instance, almost all my British figures studied in Edinburgh. What, then, did they know of George Combe (1788-1858)? Combe was not a medical man but he was a moral philosopher as well as a practitioner at law. Did Dugald Stewart, another moral philosopher, or any of the medical students who attended Stewart's lectures, have contact with Combe either before or after he began developing his own system of phrenology? As we have seen, Roget carried on a literary debate with him, earning himself the wrath of all phrenologists. Carpenter also had things to say about phrenology. Both men were well acquainted with its theory.

I cannot refrain here from mentioning a delightful little book entitled *A New Year's gift for the medical profession*,[35] published in December, 1846. It contained two essays. The first, witty and skillful, was on 'Dr. Carpenter and the antiphrenological physiologists', by T.S. Prideaux. Right in the middle of that essay is a paragraph on force which seems to have owed a great deal to some of the figures in my thesis, including Carpenter himself. Part of the paragraph goes thus:

> "The step from the inorganic to the organic world is marked by the simple elements of matter passing under the dominion of a new *force* more powerful than chemical affinity, by virtue of which they enter into ternary and quaternary combinations. There can be no doubt, however, but that this new *force* exhausts itself in its struggle against chemical affinity, so as to require to be constantly renewed by nutritive matter. The vital principle or force is therefore elaborated by a certain combination of the simple elements of matter. The essence of vitality is, then, the power inherent in a certain form and arrangement of matter, when placed under the requisite conditions regarding caloric, light and electricity, of elaborating out of the simple elements of matter the vital principle, that is, a force similar to itself. Of all the agents which influence the affinities of the elements of matter and produce

> changes in their combinations, none is so powerful as *caloric*, and its power affects the organic equally with the inorganic world".

It was tempting to follow up the author's career and enquire whether he and Carpenter might have influenced one another. However, that would have taken me into the realm of phrenology itself and a stop had to be called somewhere. I have made this comment merely to mention that I am aware of the necessity of investigating the possible relationship between phrenology and the growth of dynamical physiology.

William Paley

Having examined the place of power in 17th and 18th century British philosophy in Chapter 1, it would not be inappropriate to discuss, albeit briefly, one particular and most influential theological and philosophical treatise of the early 19th century in which power loomed large. Thus we might see how much of the earlier philosophy of power was still in use, and see the dynamical ideas of the 19th century physiologists in broader context.

The most suitable single work of the early 19th century to examine for the broad philosophical basis of its natural philosophy must surely be William Paley's *Natural Theology; or evidences of the existence and attributes of the Deity. Collected from the appearances of Nature* (1802).[36] The readership of this book was immense; at least an entire generation of the educated members of British society read and was deeply impressed by it; for instance, until his voyage on the Beagle, Charles Darwin considered Paley's book to have been the most influential single work in his education, and within the first twenty years of its appearance it went through as many editions in English.

As we might expect, Paley was much concerned with power since it was one of the traditional attributes of God and was the means whereby God's providence could manifest itself on earth. He declared, against Hume, that power really exists and that it is the sole reality behind the so-called 'laws of Nature'.[37] Such laws, he said, are only man-made; it is a perversion of language to declare any law the efficient, operative cause of anything, for a law presupposes an agent or power, which power alone is the efficient cause. Similar strictures applied to the word 'mechanism':

> "Mechanism is not itself power. Mechanism without power can do nothing. Let a watch be contrived and constructed ever so ingeniously; be its parts ever so many, ever so complicated, ever so finely wrought or artificially put together, it cannot *go* without a weight or spring, i.e. without a force independent of, and ulterior to, its mechanism. ... when we see the watch *going*, we see proof of another point, viz., that there is a power somewhere, and somehow or other applied to it; a power in action; - that there is more in the subject than the mere wheels of the machine; that there is a secret spring, or a gravitating plummet; in a word, that there is force and energy, as well as mechanism".[38]

Such confidence in the reality of power was, of course, essential for any prolonged investigation of dynamics in physics and physiology. Men do not seek and study things in which they do not believe; and for the British natural philosophers of at least the first half of the 19th century, Paley was a most eloquent champion of the reality of power and of man's duty to investigate it. As Leslie Stephen wrote in his monumental *History of English thought in the eighteenth century* (first published 1876), Paley's book was

> "a marvel of skil~~l~~ful exposition. It states with admirable clearness and in a most attractive form, the argument which has the greatest popular force and which, duly etherialised, still passes muster with metaphysicians. Considered as the work of a man who had to cram himself for the purpose, [and cram himself with remarkable thoroughness he did], it would be difficult to praise his literary merits too highly. The only fault in the book, considered as an instrument of persuasion, is that it is too conclusive".[39]

Stephen and other commentators on Paley, however, have tended to emphasize the structural features in Paley's arguments, rather neglecting its functional features. Thus, they have discussed Paley's idea that since from the intricacy of a watch we infer the existence of a watchmaker, so from the intricacy of the eye we must infer the existence of an eyemaker, the God of Nature. Thus, the common interpretation of Paley's argument is that from the intricacies of the structures, of the mere mechanisms, in Nature, man can infer the existence of God. But it seems to me that Paley's thoughts went further than this, for as we have just seen in Paley's extract above, mere mechanisms, how intricate soever it might be, was useless and relatively meaningless to him.

Only by *doing* something, by becoming the tool of some power, did it become useful and acquire its true meaning in his natural theology. *Power and mechanism together constituted the basis of Paley's natural theology, and not mechanism alone.* Hence:

> "The Divine 'omnipresence' stands, in natural theology, upon this foundation. In every part and place of the universe with which we are acquainted, we perceive the exertion of a power which we believe, mediately or immediately, to proceed from the Deity. For instance, in what part or point of space that has ever been explored, do we not discover attraction? In what regions do we not find light? In what accessible portion of the globe do we not meet with gravity, magnetism, electricity; together with the properties also and powers of organized substances, of vegetable and animal nature? Nay, further, we may ask what kingdom is there of Nature, what corner of space, where do we not fall upon contrivance and design? ... Now, an agency so general that we cannot discover its absence, or assign a place in which some effect of its continued energy is not found may, in popular language at least, ... be called universal; and ... the person or being in whom that power resides, or from whom it is derived, may be taken to be omnipresent. He who upholds all things by his power may be said to be everywhere present".[40]

The world's powers were thus envisaged as manifestations of God's Will and Power, and in him they were correlated - just as Locke, Reid and Stewart had argued. For Paley, as for Locke, this was the best argument for the existence of physical and organic powers:

> "It may likewise be acknowledgedthat no arguments we are in possession of exclude the ministry of subordinate agents. If such there be, they act under a presiding and controlling will, because they act according to certain general restrictions, by certain common rules and, as it should seem, upon a general plan...."[41]

However, exclusive interest in these subordinate agents, the physico-chemical powers, without acknowledging their divine origin was to be condemned. Such materialstic philosophers, Paley said, in resolving all things into unconscious energies have not really advanced upon the ancient forms of atheism. It was the duty of a Christian philosopher to keep the presiding power of God constantly in view when investigating the powers of Nature. Men like Davy, Faraday, Roget, Carpenter and even Draper must have agreed whole-heartedly with him on this point.

Another point on which they surely agreed with him and which also concerned power was that of 'relations'. Chapter XV was called simply 'Relations', discussing the fitness or relations among natural phenomena; and although Paley did not emphasize the relations among Nature's powers he did have this dynamical feature somewhere in his mind. This is most evident in Chapter XVII, on 'The relation of animated bodies to inanimate nature'.

"Take the earth as it is", he writes,

> "and consider the correspondency of the powers of its inhabitants with the properties and condition of the soil which they tread. Take the inhabitants as they are; and consider the substances which the earth yields for

> their use. They can scratch its surface; and its surface supplies all which they want. This is the length of their faculties: and such is the constitution of the globe, and their own, that this is sufficient for all their occasions".[42]

The very study of relations, let alone their actual existence in Nature, depended on Nature being mechanical. Such mechanism was especially important for those physiologists who were most interested in relations between the animate and inanimate and who sought dynamical connexions between them. Indeed, only within a truly mechanical world could the idea of a correlation of forces have a truly useful rôle, for if one were to deny the mechanical nature of living things (at least as a working hypothesis), the investigation of their functional relations with the rest of creation would be severely limited. What was even more important in Paley's opinion, (and Carpenter reiterated this),[43] was that if a uniquely vital force, independent of all other forces, were supposed to be the prime mover in living organisms, they would no longer depend on God's own power, and the great chain of power would be broken. Vitalism was the high road to atheism.[44]

I shall make one more comment on Paley's *Natural theology* with regard to its likely impact on the physiologists we have examined. Paley emphasized that his argument of design in the living organism was essentially different from arguments in physical science. The latter rely upon each step in the argument being valid, one step being false and the entire edifice collapses. But Paley saw his argument as cumulative, so that the overall conclusion did not depend critically on the soundness of each part:

> "Of this point /of design/, each machine is a proof independently of all the rest. So it is with the evidences of a Divine agency. ... it is an argument separately supplied by every separate example. An error in stating an example affects only that example. The argument is cumulative, in the fullest sense of that term".[45]

Let us recall that Carpenter concluded his Royal Society paper in 1850 on exactly this theme - that his argument should be treated cumulatively. Charles Darwin was also to emphasize the cumulative nature of his argument in the *Origin of Species*. Indeed, physiologists not infrequently admitted that their hypotheses could not but have that character, for the life-sciences were (and perhaps still are) not amenable to rigorous, linear arguments, but must needs employ cumulative ones.

It seems to me that this difference between the biological and the physical sciences is part of the reason why historians of science, as well as most mid-late 19th century commentators on dynamics, have focussed on the contributions of physical scientists to the energy question, to the general exclusion of physiologists. The latters' arguments could never hope to be conclusive, in the sense that an argument by a physical scientist could be, simply because of the complexity of the living organism. Yet to other *physiologists*, the ideas on force that Carpenter, say, or Draper, advanced, might well have seemed conclusive. This point is also relevant with respect to men like Helmholtz, Mayer and Liebig who all worked, at least initially, within a physiological *milieu*, for they must have been impressed by ideas on force that their German predecessors in physiology had advanced.

The apparent rigour of physico-chemical argument might also explain why the metaphysical and theological contributions to the energy doctrine have been largely ignored, at least so far as the energy doctrine in Britain was concerned. One of my modest aims has been to draw attention to that philosophical content. I have not attempted to evaluate its importance compared with the purely physico-chemical research that was done on power. All that I have done has been to give a glimpse of the philosophical background behind the work of the natural philosophers and physiologists in the English-speaking world.

If one extract is to be chosen from the works of the British contributors to the energy doctrine in the 1840s, in order to illustrate the philosophical fascination of the subject, no better choice could be made than William Robert Grove (1811-1896), the cautious physicist and barrister. To Grove is usually accredited the full and experimentally-based enunciation of the correlation of forces. His work was unquestionably scientific. Yet the metaphysics of power was part of his scheme, as we see at the very end of a paper 'On the gas-voltaic battery',[46] read to the Royal Society in 1843. There he suggested that powers might be mere constructs of the human mind, since all natural phenomena could be reduced to

> "effects of *motion and matter*. These two seem the most distinct, if not the only conceptions of the mind, with regard to

> natural phenomena, and when we try to comprehend or explain affections of matter, which are not obviously modes of motion, we hypothetically or theoretically reduce them to it: The senses perceive the different effects of sound, light, heat, electricity etc., but the mind appears capable of distinctly conceiving them only as modes of motion. Does not this supply an argument that all physical agencies are reducible to these elements of mental conception? Or are we to look for new powers of mind, in other words, will greater familiarity with phenomena, at present recondite, enable the mind more clearly to comprehend them, and avoid the necessity of referring them theoretically to more familiar and apparently more similar phenomena? To pursue this curious inquiry would involve me in a discussion foreign to the object of this paper, and to the general character of contributions to the Royal Society, but the question arises so immediately out of the subject, and is so necessary to explain my own view, that I trust this brief statement of it will be considered sufficiently pertinent. It touches upon that interesting, scarce definable boundary, where physical merges into metaphysical science".[47]

Such was the nature of power - a problem for two sciences, namely physics and metaphysics. As we have seen in these past nine chapters, various, sometimes quite 'scientific', natural philosophers and physiologists sensed the import of the power question and tackled it in different ways. From the speculative generalities of John Brown through to the learned sobriety of William Carpenter, from Locke to Paley, there ran a common thread which ended in the mid-century doctrines on force and energy. Another thread, that which belonged to chemists and physicists of the period, crossed the first on many occasions, and we can best see their crossing (and their staying together) in Carpenter's marathon paper to the Royal Society in 1850. So far as Carpenter was concerned and, in my opinion, so far as dynamical physiology in Britain was concerned, that paper marked the end of an era.

NOTES TO CHAPTER 9.

1. R. Fowler, *Experiments and observations relative to the influence lately discovered by M. Galvani, and commonly called animal electricity*, Edinburgh, T. Duncan and others, 1793.

2. *Ibid.*, p.63.

3. *Ibid.*, pp.101-102.

4. R. Fowler, 'If vitality be a force having correlations with the forces, chemical affinities, motion, heat, light, electricity, magnetism, gravity, so ably shown by Professor Grove to be modifications of one and the same force', in *Report of the nineteenth meeting of the British Association for the Advancement of Science, held at Birmingham in 1849*,London, J. Murray, 1850, pp.77-78 in 'Transactions of the sections'.

5. *Ibid.*, p.78.

6. *Report of the twenty-ninth meeting of the British Association for the Advancement of Science, held at Aberdeen in September, 1859*, London, J. Murray, 1860, pp.160-162 in 'Transactions of the sections'.

7. *Ibid.*, p.161.

8. *Ibid.*, pp.161-162.

9. Richardus Fowler, Anglus 'De inflammatione' in *List of the graduates in medicine in the university of Edinburgh, from MDCCV to MDCCCLXVI*, Edinburgh, Neill & Co., 1867, p.24.

10. G.C. Holland, *Experimental inquiry into the laws of organic and animal life*, Edinburgh, Maclachlan and Stewart, 1829.

11. G.C. Holland, *Inquiry into the principles and practice of medicine, founded on original physiological investigations*, London, Longman, Vol.1, 1834, Vol.2, 1838.

12. *Ibid.*, p.32.

13. *Ibid.*, p.11.

14. *Ibid.*, pp.34-35.

15. *Ibid.*, pp.492-494.

16. *Ibid.*, p.492.

17. *Ibid.*, p.495.

18. *Ibid.*, p.497.

19. *Ibid.*, vol.2, p.123.

20. *Ibid.*, p.204.

21. *Ibid.*, p.135.

22. *Ibid.*, p.136.

23. O.S. Fowler, *Works on phrenology, physiology, etc.*, Joseph Barker (editor), London, J. Watson, 1850.

24. J.S. & R.M. Haller, *The physician and sexuality in Victorian America*, Urbana, Univ. of Illinois Press, 1974, esp. pp.159-164, 209-210; and 227 for his phrenology.

25. O.S. Fowler, 'Amativeness; or evils and remedies of excessive and perverted sexuality', in *op.cit.*, Note 23 above. Each pamphlet has its own pagination, there being no continuous pagination for the whole book.

26. *Ibid.*, p.7.

27. *Ibid.*, p.8.

28. *Ibid.*, p.26.

29. O.S. Fowler, 'Love and parentage', in *op.cit.*, Note 23 above, p.7.

30. *Ibid.*, p.7. He also refers one to the *American Phrenological Journal* for 1845, pp.129 and 359.

31. O.S. Fowler, 'Physiology, animal and mental: applied to the restoration and preservation of health of body and power of mind', in *op.cit.*, Note 23 above, pp.106-107.

32. O.S. Fowler, 'Temperance, founded on phrenology and physiology', in *op.cit.*, Note 23 above, p.18.

33. *Op.cit.*, Note 31 above, p.29.

34. C. Caldwell, *Physiology vindicated in a critique on Liebig's Animal Chemistry*, Jeffersonville, Indiana, A.S. Tilden, 1843.

35. T.S. Prideaux & W.C. Engledue, *A New Year's gift for the medical profession*, London, H. Baillière, 1847.

36. W. Paley, *Natural theology; or evidences of the existence and attributes of the Deity. Collected from the appearances of Nature*, London, J. Faulder, 1802. I have used the 12th edition, London, J. Faulder, 1809.

37. *Ibid.*, p.7.

38. *Ibid.*, p.417.

39. L. Stephen, *History of English thought in the eighteenth century*, 2 vols., first published 1876. I have used the London, Harbinger books, 1962 edition. This quotation is from vol.1, p.346.

40. *Op.cit.*, Note 36 above, pp.445-447.

41. *Ibid.*, p.454.

42. *Ibid.*, pp.294-295.

43. W.B. Carpenter, *op.cit.*, Notes 52 & 53, chap.7 above.

44. It seems to me that this explains Paley's rejection of Lamarck's theory of organic development. For instance, see pp.162-163. Also see my discussion on T. Southwood Smith in Chapter 5, esp. Note 46, and the opinions of Draper in Chapter 8.

45. *Ibid.*, p.77.

46. W.R. Grove, 'On the Gas-Voltaic battery', *Phil.Trans.*, 1843, pp.91-112.

47. *Ibid.*, p.108.

CHAPTER 10. On Power, Causality and Relation in the principal Continental philosophies from Des Cartes to Schelling.

So far as German natural philosophers in the first half of the 19th century were concerned, the philosophical analysis of force and power, (which were usually treated synonymously, but were differentiated occasionally), began with Gottfried Wilhelm Leibniz (1646-1716). However, he cannot be understood without a brief look at the philosophy of René Des Cartes (1596-1650), which he took as his basis and subsequently modified - sometimes beyond all recognition. Des Cartes is therefore my starting-point.

I should interject a humble warning: what follows is not an attempt to describe exhaustively the places of power, causation and relation in these philosophical systems. I would not have written anything on such themes, had I not come across frequent, and often lengthy, discussions of them in the writings of the German physiologists who will be enumerated in subsequent chapters. In order to help myself to understand the philosophic framework within which those physiologists operated, some study of secondary philosophical literature at least seemed essential; it then seemed that the presentation of those physiologists' ideas on power would be best achieved by writing a preliminary synopsis of what the philosophers had taught them - on 'causation' and 'relation' as well as on 'power'. Such is this chapter.

This chapter has relied greatly on secondary material, especially the following: Bertrand Russell's *History of western philosophy*,[1] which contains fine philosophical discussion despite occasional historical inaccuracies and its rather subjective, sometimes flippant, interpretations; Robert Adamson's dated but still respected *Development of modern philosophy*;[2] Pierre Costabel's *Leibniz and dynamics*;[3] John Kemp's *The philosophy of Kant*,[4] which is both lucid and detailed; H.J. de Vleeschauwer's *The development of Kantian thought*,[5] which is especially useful in detailing the differences between Kant's first and second editions of *Kritik der reinen Vernunft*; C.A. von Peursen's *Body, soul, spirit: a survey of the body-mind problem*;[6] and Gottfried Martin's *Kant's metaphysics and theory of science*.[7] I also learned much from the lengthy philosophical discussions that Johannes Müller and Hermann Helmholtz included in their scientific treatises, and although well aware of the likely inaccuracies and prejudices in their discussions, they seemed to be fair and accurate on the whole.

My primary source reading has been in Des Cartes (almost all his published material), Leibniz and Kant (only the *Kritik der reinen Vernunft*.[6]).

Des Cartes had scarcely any use for the concept of force in his natural philosophy, for the essential and primary attribute of matter was extension; the other properties of matter, including its dynamical ones, were derivative. However, power was important for things simply to exist at all, for existence depended directly on the will and power of God, since God was the First Cause.[9]

Much of Des Cartes' philosophy and method was taken up by Benedict Spinoza (1632-1677), and synthesized into a novel, and at times startling, system. The question of causation was a central problem in Spinoza's system; he rejected it, rejected the traditional idea of God as the First Cause, and in place of causation substituted the type of relation which is found in geometry, namely, that of ground to consequent. What he sought was therefore the supreme ground of all things, namely that assumption which had to be made, in order to render intelligible the assertion of anything else.[10]

Within the process of acquiring knowledge, Spinoza made a fundamental distinction between the understanding and the imagination. To imagination he referred all familiar and easy conceptions of things, which prevent us from contemplating the complete symmetry of logical arrangement that pure reason demands; simply because we consider things with our imaginations, we conceive of them as contingent and variable, and not determined by a merely logical sequence; hence, all those familiar ideas of connexion, such as locality in space and causation, were, for Spinoza, misinterpretations of the real, logical sequences in Nature. God could not be a cause of the universe, but he was the supreme ground or *raison d'être* upon which all else was consequent. These consequents were not to be supposed independent of the ground, but formed part of the full conception of the ground itself.[11] Therefore, the whole of existence constituted a unity, and the study of any one part of Nature would be relevant to all other parts and would be a set of common principles.

None of the physiologists whom we shall discuss adhered to Spinoza's denial of causation; but at least one of them, Johannes Müller, applauded his doctrine of the divine unity of Nature, or Pantheism; and his pupil, Hermann Helmholtz saw fit to include a long analysis of Spinoza in his *Treatise of physiological optics*;[12] indeed, of the two philosophers who seem most to have informed Helmholtz's metaphysics, as evidenced in his scientific as well as his philosophical writings, Spinoza was one; Kant was the other.

Spinoza's influence was exerted in great measure through his impact on Leibniz. As Bertrand Russell has shown,[13] there were two philosophical systems that Leibniz constructed - the one which he published and which students of his thought read until the early 1900s, and the one which he dared not publish; both were influenced by Spinozism, even though Leibniz denied such influence; here, we shall be looking largely at his published system.

On arriving in Paris in 1672, Leibniz fell under the influence of Cartesianism, though he soon acquired serious misgivings about it; one cause of his dissatisfaction was that Des Cartes had apparently excluded the possibility of final causes, or teleology, from Nature. Spinoza also had grave misgivings on that score, and in 1676 Leibniz visited Spinoza to discuss their difficulties and exchange ideas.[14]

In 1677, Spinoza's *Ethics* was published posthumously; Leibniz acquired a copy immediately but found no solutions therein to his dissatisfaction with Des Cartes; in fact, Spinoza seemed to deny final causes more strenuously than Des Cartes, and as Adamson has pointed out, Liebniz had to turn to Plato for a reaffirmation of teleology.[15]

His dissatisfaction with Cartesian physics came to a head with Des Cartes' principle of the conservation of motion in the universe. Leibniz, among others, wanted a clearer definition of 'quantity of motion', and since he was a fine mathematician and protégé of Christiaan Huyghens (1629-1695), Leibniz was able to supply one himself, in the form of *vis viva*. In so doing, he included one parameter that Des Cartes had proscribed, namely inertia or mass, his main argument for its inclusion being that the logical consequence of Des Cartes' view would be the admission of perpetual motion. Perpetual motion was no less desirable to the Cartesians than to Leibniz himself, and French, German and Swiss mathematicians throughout the 18th century argued powerfully against perpetual motion in the inorganic world.[16] Let us note, however, that those arguments did not consider the possibility of such motion in organic beings; that task was apparently left exclusively to physiologists, for there was no *a priori* reason to suppose that organic systems should follow the same principles of motion as inorganic ones. Among the physiologists who tackled that task, we shall discuss in the following chapters Christoph Wilhelm Hufeland (1762-1836), Johann Friedrich Blumenbach (1752-1840), Friedrich Tiedemann (1781-1861), Johannes Müller (1801-1858), Justus Liebig (1803-1873) and Hermann Helmholtz (1821-1894); one physiologist who will scarcely be mentioned, although he tackled the same task, is Julius Robert Mayer (1814-1878): there simply will not be room to discuss his ideas adequately. Some of these physiologists actually saw their task as bringing the study of physiological motion up to the level of sophistication and certainty that had been attained for inorganic motion: for instance, at the beginning of his 1845 paper on 'The motions of organisms and their relation to metabolism', Mayer lamented how the discoveries of Galileo, Newton and Mariotte had borne no fruit in physiology, and that despite the work of men like Theodore Schwann (1810-1882) and Gabriel Gustav Valentin (1810-1883) on muscle action, there was still a huge gap between physics and physiology in the study of motion.[17]

With Leibniz's concept of *vis viva*, or active force, we come to a definite distinction between what we nowadays call "force" and "energy". Various historians of science[18] have discussed this distinction, the essence of which was that Des Cartes was concerned solely with a body's momentum (mv), whilst Leibniz was concerned with the effects or workings of a force-in-action. As Leibniz explained in a paper in 1695, he considered the cartesian force to be dead force, or *vis mortua*, because motion did not yet exist in it, but only the instigation towards motion; by contrast, his own *vis viva* was intimately associated with motion; *vis viva*, indeed, was produced by an infinite number of applications of dead force.

It is well known that Christiaan Huyghens had used the product of mass times the square of velocity (mv^2) in his analysis of the compound pendulum in 1673, but he did not envisage it as a fundamental expression that might have more general application. Leibniz, however, singled it out as a new and important expression and gave it a name - *vis viva* - which stuck with it until well into the 19^{th} century when it was renamed kinetic energy ($\frac{1}{2}mv^2$). Once Leibniz had proposed that Des Cartes' principle of the conservation of the quantity of motion (mv) in the universe might be replaced by a law of the conservation of *vis viva*, it was not long before Huyghens saw that *vis viva* was related to the concept of work itself. From Huyghens' law $v^2 = 2as$, and Newton's law $f = ma$, it was easy to see that $fs = \frac{1}{2}mv^2$. In Leibniz's terminology, *vis viva* was the continuous sum /i.e. the integral/ of infinitely many *vires mortuae*.[19]

Leibniz's concept and precise formulation of *vis viva* suffered various fortunes at the hands of 18^{th} century mathematicians, like Johann Bernoulli (1667-1748), Jean Le Rond d'Alembert (1717-1783), Leonhard Euler (1707-1783), Daniel Bernoulli (1700-1782) and others; but whatever they thought about *vis viva* as a real existent - d'Alembert, for instance, denied that it really existed as a force - they all agreed that the mathematical principle of its conservation seemed to hold.

So far as Huyghens and Leibniz had been able to demonstrate, the conservation of *vis viva* applied only to elastic impact; but Leibniz, being as eager as Des Cartes had been to construct a metaphysical as well as a physical system, suggested that the conservation of *vis viva* might be a universal principle applying to both inelastic and elastic impacts; in the former, *vis viva* which apparently vanished merely assumed a different form - it became latent. As Carl Boyer has pointed out,[20] this was not unlike Joseph Black's later concept of latent heat, but unfortunately Leibniz could not supply quantitative verification.

Leibniz's doctrine of *vis viva* may surely be called a forerunner of the 19th century Conservation of Energy, despite its largely metaphysical character, its lack of empirical evidence, and its primary concern with motion only. It is important, for instance, to realize that Leibniz and some of his adherents envisaged *vis viva* to be more general than the dynamics of mere motion; for them, it was a truly universal principle; we see this in an extract quoted by Thomas Kuhn from Daniel Bernoulli's *Hydrodynamica* of 1738:

> "I am persuaded that if all the *vis viva* hidden in a cubic foot of coal were called forth and usefully applied to the motion of a machine, more could be achieved than by the daily labour of eight or ten men".[21]

One aspect of Leibniz's philosophy which ought to be mentioned, since it dealt with a problem which 19th century writers on force felt acutely, was his view of definitions. A definition was said to be real in two cases: i) When its content can be shown to be logically possible; or ii) When experience corresponds with its content. A definition was merely nominal when it only assigned a meaning to a word, without deciding whether its contents were possible or actual.[22] As is well known, a large part of Newton's controversy with Leibniz,[23] especially as conducted in the Clarke-Leibniz correspondence, concerned the definition of Newton's force of gravitation; and this concern over the status or definition of force was shared by 19th century natural philosophers. It seems to me that there might well be a connection between the attitude of those contributors to the conservation of energy mentioned by Kuhn, whose "notion of an underlying, imperishable, metaphysical force seems prior to research and almost unrelated to it", and Leibniz's view that a definition could be real simply if it was logically possible.

Another theme that Leibniz discussed, and which was important in the 19th century debates on force, was relations. Obviously taking his cue from Spinoza, he asserted that relations between things are not external to those things, but that the relations must be contained within the natures of the things themselves. On the cosmic scale, since all things are related to one another, for instance by possessing spatial relations or simultaneity, and since the formation of the idea of any individual being is dependent on the formation of the ideas of the rest of the universe, the idea of each individual implies, or more exactly contains, the ideas of all the rest. The implication of this objective connexion of all things was that each unit of the whole, each monad, is in itself a complete world.[24]

One property that Leibniz would not attribute to these units was that *sine quâ non* of Des Cartes, namely extension, since

> "Every extended mass may be considered as
> composed of two, or a thousand, others;
> ... so that one can never find a body which

> can be truly called a substance. It will always be an aggregate of several substances .... Extension is an attribute which cannot constitute a complete being; no action or change can be derived from it; it expresses a present state only - not the future and the past, as the notion of a substance should do".[25]

Consequently,for Leibniz the one admissible property of things was force or power - that by which activity or change could occur. However, realizing that the purely abstract conception of power was of little avail to his scheme of things, Leibniz took up and developed a proposition of early Greek metaphysic - that unity and reality are one and the same.[26] Furthermore, since the essential property of reality was active force, or *vis viva*, to be a "one" was synonymous with activity. Thus, the type of physical unit or atom that Des Cartes, Gassendi and others had in mind, was not a true unit for Leibiz; in this he was followed by Roger Boscovich and various natural philosophers of the early 19th century. The core of Leibniz's view of reality was therefore as follows: The characteristic of reality is definable by two marks - unity and activity. To exist and to act amount to the same thing; and each individual existent can be represented solely through activity or active force. Of the Continental philosophers, therefore, it was Leibniz who first wrote the full credentials of force and power for the use of natural philosophers; and to him, men like Johannes Müller (see Chapter 13), Hermann Helmholtz (see Chapter 16) and Julius

Robert Mayer[27] referred in their physiological writings.

Immanuel Kant (1724-1804)

Of the three principal errors into which Kant believed metaphysics had fallen,[28] we shall here discuss only one, namely rational cosmology. In this branch of metaphysics, it seemed to him that conflicting arguments had often been proposed, and yet on the commonly accepted metaphysical assumptions, they had been incapable of resolution. These conflicts, or 'antimonies' as Kant called them, seemed to be inherent in reason itself; Kant believed that they could be resolved, but only by a long and penetrating re-appraisal of philosophy:[29] that was the purpose of his *Kritik der reinen Vernunft*. Of the four antinomies that Kant diagnosed in cosmologies, two were concerned with causation. Briefly, they were:

1. Thesis: Causality according to laws of Nature is not the only type of causality from which the phenomena of the world can be derived. These phenomena also necessitate a 'causality of freedom'.
Antithesis: There is no freedom; everything in the world occurs according to the laws of Nature.

2. Thesis: There belongs to the world, either as a part of it or as its cause, a being who is absolutely necessary.
Antithesis: No such being exists.[30]

In order to resolve these antimonies, Kant undertook a full inquiry into the nature of man's reasoning powers and their suitablity for their task. Of course, Des Cartes had claimed to do much the same thing, but his had not been the systematic attempt that Kant believed necessary. Locke had come rather nearer the mark, for in his *Essay concerning human understanding* he had attempted to distinguish between those issues on which the human mind could attain certainty, and those on which it could acquire only opinions. Power was one of the concepts of whose general existence men could be certain.[31] Kant appreciated the sincerity of Locke's effort to unravel "the physiology of the human understanding", as he put it,[32] but believed that it had not probed deeply enough and had been limited by Locke's empiricism.

The only one of his precursors whom he accredited with making a substantial contribution to the task was David Hume; indeed, he asserted that Hume had first awakened him from his dogmatic slumbers.[33] Kant considered Hume's principal achievement to have been his analysis of causality, in which he had shown that reason alone, working from *a priori* concepts, could not prove that a particular effect results necessarily from a specific cause; such a cause-effect relation could be discovered only by experience, in Hume's view, and therefore reason was not entitled to lay down *a priori* laws of connexion. As Kant put it:

> "The imagination, having by experience brought certain representations under the law of association, passes off a subjective necessity arising out of this, namely custom, for an objective necessity from insight".[34]

Kant thought that in arriving at this sceptical conclusion Hume had been consistent, unlike Locke who, though admitting that our ideas are derived from experience, still tried to use them ot transcend experience. Nonetheless, Kant had three specific criticisms of Hume's system:[35] firstly, it dealt only with the concept of causality and not with other concepts of the understanding too; it was insufficiently general. Secondly, having ascertained what human reason could not do, it had not discovered what reason might do. And thirdly, it had not distinguished between the validity of the belief that A caused B, and the belief that B must have had some cause, whatever that cause may be; Kant asserted that the first proposition can be known only from experience, whilst the latter is an *a priori* one.

If true metaphysical knowledge were to exist, Kant believed that it would have to be constructed upon *a priori* general concepts to be truly scientific. He envisaged three sciences which could yield *a priori* knowledge - logic, mathematics and physics[36] - and enquiring what it was that allowed them to be called sciences, he concluded that they alone used *a priori* judgements in a way that was philosophically rigorous. The use of such judgments in pure logic was easy to defend since its method of reasoning was purely analytic and could be tested merely by the law of contradiction. Mathematics and physics were more difficult to defend since they provided synthetic *a priori* knowledge; since this was what metaphysics would also have to do, it seemed to him that to become a science, metaphysics, and any other field for that matter, would need to acquire synthetic *a priori* knowledge as a prerequisite. These issues were handled in the principal part of his *Kritik*: in the sections on

"Transcendental Aesthetic" and "Transcendental Analytic" he explained how synthetic *a priori* judgements were possible and how the sciences of mathematics and physics were based thereon.

In Kant's view, man's empirical knowledge has two sources - sensibility and understanding. One of his most important ideas was that the sense-impressions, which convey information from the outside world into man's sensorium, are not already organised into intelligible categories, such as spatial relations, but that the human mind organizes them thus. For instance, we receive the light-rays from a book, but it is not given to us that this object is a book; we must first form for ourselves the concept of book. Kant's task was therefore to explain the intellectual, formalizing aspect of sense-perceptions, rather than the other aspect, namely physical sensation, which was no less important but was easier to grasp. (This latter aspect was to be tackled by a series of physiologists and natural philosophers in the next century, to wit Johannes Müller who, however, followed Kant's assertion of the importance of both the physiological and the psychological aspects of sense perception, and expressed a belief in the necessity of a physiological psychology in his pioneer treatise on dreams;[37] Hermann Helmholtz; the Czech physiologist, Jan Evangelista Purkyně (1787-1869); and, at the end of the century, Ernst Mach (1838-1916), the physicist-philosopher.)

Kant set out to show that, just as the nature of our sensibility ensures that all it receives is spatially and temporally arranged by it, so the nature of our understanding ensures that all knowledge, empirical as well as *a priori*, is arranged according to certain rules or categories; that the spatio-temporal character of the world is derived from the formal character of our faculties of sensation; and that our conviction, for instance, that every change has a cause, is derived from the formalizing character of our minds.[38]

There is a lot more that could be said on Kant's view of apprehension, but the features that were to be important for investigators like Müller can be summarized as follows: Nature is subject to the categories of the intellect, not because things in themselves are so arranged (things in themselves, *noumena*, have their own arrangements, which we can never know), but because Nature is nothing for us except as it appears to us. Nature as it appears to us is, therefore, the only level on which we may usefully talk about and investigate reality. And the way in which it appears is determined by the fact that our intellects have to categorize our perceptions, if we are to make any sense out of them. This does not mean that we can discover the operations and details of Nature by intellectual activity alone; we cannot deduce scientific knowledge merely from the categories. It does mean that, although empirical investigation is necessary before scientific knowledge can be acquired, the general structure of that knowledge is determined by the human mind.

Friedrich Wilhelm Schelling (1775-1854) and some *Naturphilosophen*

It is part of my argument in the following chapters that the notion of the conservation and correlation of forces, as it was held by men like Müller, Helmholtz, Liebig and even von Reichenbach, was considered by them as one of the *a priori*, synthetic judgements of the human mind, and that, as Kant had argued, it could be a thoroughly respectable scientific concept even before it had been verified empirically, (which, need it be said? it never can be). Similarly, they believed that forces are real entities, even though they could not be demonstrated in any tangible way; the reality of a force was sufficiently assured by a certain set of phenomena which seemed to bespeak a common ground or cause, and since the human mind could perceive only phenomena, that was all that was required. (See the chapter on Liebig especially for an illustration of this argument). This argument is one aspect that I have chosen to investigate, of Kuhn's suggestion that *Naturphilosophie* provided a powerfully fertile basis for the emergence of conservation of energy. As he has mentioned, the *Naturphilosophen* took the concept of organism as the fundamental metaphor for their universal science, and consequently sought unifying principles for all natural phenomena.[39] The high-priest of *Naturphilosophie*, Friedrich Wilhelm Schelling, constructed his philosophy largely upon Kant's system and the extensive modifications of that system by Johann Gottlieb Fichte (1762-1814).[40] In his philosophy of Nature, Schelling, like Fichte, was essentially concerned with processes of development; and in both Nature and Spirit (which were his twin aspects of reality), the principle of development was essentially Thought. Nature, for example, was supposed to exhibit the gradual development of what may be called slumbering thought, natural force, in three ways, each of which contained the antithesis required for any activity: the first way was mechanism, where the antithesis was between attractive and repulsive forces of inorganic matter; the second way was light, itself containing the activities of magnetism, electricity and chemical force, in which the general phenomenon of polarity constituted the antithesis; and the third way was organic life, where the antithesis was manifested by the fundamental processes of reproduction, irritability and sensibility. Thus, he maintained that magnetic, electrical, chemical, and finally even organic phenomena would be woven into one great association extending over the whole of Nature; consequently he was forever on the look-out for processes of conversion and transformation.[41] Schelling's natural philosophy appeared first in print in 1797 with his *Ideen zu einer Philosophie der Natur*; in 1799 appeared his *Erster Entwurf eines Systems der Naturphilosophie*, and the last major writing on that theme appeared in 1809. Thus this stage of his intellectual development occupied only about 12 years in the first half of his life. Schelling's teaching came to dominate most German-speaking universities during the first third of the 19th century. Indeed, his philosophy of Nature was well received not only by poets and *literati* of the Romantic School, but also by eminent natural philosophers like Lorenz Oken (1779-1851), Karl Ernst von Baer, and Karl Friedrich Burdach (1776-1847). The physicians of the Brownian School, who viewed man as a unity of body and soul which should always be treated together, welcomed his ideas. In 1802, the medical faculty of Landshut University awarded him an honorary doctorate in medicine. (One of the figures in this thesis, Friedrich Tiedemann, had worked with Schelling in Würzburg in the early

1800s and in 1807 was called to Landshut as professor of anatomy and zoology).

Schelling's philosophy was therefore disseminated widely and was available to at least a whole generation of scholars. Amongst the natural philosophers who fell under his sway, Kuhn mentions Hans Christian Oersted (1777-1851), professor of natural philosophy at Copenhagen, who persisted for many years in his search for a fundamental relation between electricity and magnetism, largely because of his prior philosophical commitment to such a relation. However, Oersted recognized the danger of unbridled speculation, such as he had found in Schelling, and he emphasized the necessity of experimentation as a check.

Yet another *Naturphilosophe*, or at least another natural philosopher who was influenced by Kant, Ampère, argued that it was justifiable to speculate freely so long as one did so within a very limited, carefully defined area, and that one may speculate on the nature of things in themselves, on the *noumena*, and from such speculations reconstructing the phenomenal world. Thus he constructed a theory of electrodynamic molecules and believed them to be valid, because he could deduce therefrom the magnetic phenomena descernible in the phenomenal world of experimental physics. His model gave unity and precision to the study of electricity and magnetism, and that was its justification.[42]

Not all *Naturphilosophen* were content to leave the question of speculative hypotheses on this level. Natural philosophers like Oersted, Johann Ritter (1776-1810) and Christian Samuel Weiss (1774-1853) could not be satisfied with models which were initially arbitrary, like Ampère's. They sought the absolute truths of Nature's *noumena*. Yet how was that possible, granted Kant's rigorous distinction between phenomena and *noumena*? Inevitably, since they were devout Kantians and Kant (like all seminal thinkers) could mean all things to all men, they found their solution in the master himself. In his second antimomy of pure reason Kant had argued that it is impossible to decide between an atomic and a plenist view of the world. Yet there was a way out which was only hinted at in the *Kritik* but was developed especially in the later *Metaphysische Anfangsgründe der Naturwissenschaft*. In the *Kritik* Kant asserted that

> "We are acquainted with substance in space only through forces which are active in it /Leibniz's point/, such forces either bringing other objects to it (attraction) or preventing them penetrating into it (repulsion and impenetrability). We are not acquainted with any other properties constituting the concept of substance which appears in space, and which we call matter".[43]

The *Metaphysische Anfangsgründe* developed this epistemological point and, in one historian's opinion, thus laid the basis for the 19th century study of dynamics and thus pulled the disparate strands of *Naturphilosophie* together into a fairly unified system, enabling such diverse minds as Schelling, Ampère, Oersted, Davy and Coleridge to cohabit under the one roof. This unifying dynamical world-view was responsible for *Naturphilosophie*'s bewitching and often irresistible influence upon even the most experimental of scientists in the 19th century.

That same historian asserted that it was also from this unifying dynamical world-view that "the most important methodological innovation of *Naturphilosophie* was drawn".[44] Kant's attractive and repulsive forces, as we have seen, were adopted by Schelling, but whereas Kant had envisaged an equilibrium resulting from the conflict of forces of equal magnitude, Schelling envisaged a development into a higher level of conflict entailing new forms of force. For Schelling, therefore, everthing was ever active, such activity being sustained by God. God literally manifested himself in force which acted through spirit and matter. From the conflict of forces came beauty. Such beauty rested, of course, on God. God alone was the guarantor of the scheme's truth. Truth and beauty, therefore, derived from God's ceaseless activity manifested by conflicting forces. On such a basis could the sciences ever rest, as we can see from the only treatise that Oersted ever wrote; it was published in French with the give-away title of *Recherches sur l'identité des forces chimiques et electriques*.[45] There, as is well known, Oersted predicted the discovery which was to make his reputation - the transformation of electrical into magnetic force. His prediction was merely a corollary to Schelling's doctrine that conflicting forces, when sufficiently constrained, undergo transformations into other forces. After years of experimenting, Oersted managed to constrain a force sufficiently by passing an electric current through a thin wire of high resistance; a magnetic force appeared. This set off a search by himself and others (for instance, Davy and Faraday) for further transformations of forces. This search, which lasted from c.1820-1860, was largely unfruitful, but a few investigators such as Grove, Joule and Mayer did obtain confirmatory results. Its frequent unfruitfulness did not persuade its adherents that their belief in the unity and transformations/correlations of forces was misplaced. Take Faraday for instance. Try as he might, he never demonstrated the magnetization of light, yet he never doubted its possibility.[46] What failure meant was merely that the correct conditions for constraining light had not yet been found.

Against this complex background, in which the belief in the unity or correlation of forces was pre-eminent, the researches of the 19th century German contributors to the energy conservation doctrine must be seen, as Kuhn and other scholars have declared. Only then shall we appreciate why the young Helmholtz *expected* many of the older German natural philosophers to react to his thesis '*Ueber die Erhaltung der Kraft*' in 1847 with a sigh of "What does this fellow think he is doing? We know all that already".[47] Only then shall we also appreciate why many of them *actually rejected* his thesis, for, having been brought up on the diet of *Naturphilosophie* and then made to see how unproductive it was compared with experimental science, they were reacting against it lock, stock, and barrel. Moreover, only then shall we appreciate one of the aims of Liebig's *Animal chemistry* for, despite his denial of owing anything to *Naturphilosophie*, those passages wherein he discussed force are so eloquent, enthusiastic, even emotional, that I cannot believe that he was investigating force in the utterly empirical, sober way he wanted us to believe.

To mention one more legacy that Kant left to the 19th century, he and more especially the *Naturphilosophen* embraced the metaphor of the organism for their world-view. By and large the 18th century had been impressed by the mechanical appearance of the world, and the machine was the only metaphor that the *Philosophen* would allow.[48] But for Kant and his followers, since the concepts of final cause and development were important, and since Nature was envisaged as an harmonic whole, whose wholeness was more meaningful than the mere sum of its parts, the world had the character of an organic being. Plato's query, quoted earlier in this thesis, was again admissible: "In the likeness of what animal is the world made?" This organismic world-view would appeal inevitably to biological natural philosophers, but it also took hold of physicists. Oersted was one such, as Kuhn has shown. I would cite Alexander von Humboldt (1769-1859)[49] too, as a prime example; he wrote that a truly philosophic view of the physical world

> "... would seem to lack its attractive features, did it not at the same time present the sphere of ORGANIC LIFE in the numerous grades of its typical developments. The idea of animation is so closely connected with the idea of the existence of the impelling, ceaselessly active, decompounding, compounding and fashioning natural forces, which inhere in the terrestrial globe, that in the popular myths of the nations of antiquity, the production of plants and animals was always ascribed to these forces".[50]

As a young man Humboldt had moved in Goethe's and Schiller's circles in Weimar and Jena and he continued to proclaim their philosophy throughout his long life. We shall meet him again, *en passant* only, on two occasions in the following chapters - firstly as a student of the physiologist Johann Friedrich Blumenbach, and then as the invaluable patron of a very young and brilliant chemist, Justus Liebig.

To summarize this organismic world-view: the laws of Nature were seen as laws of development. Just as organisms live because they are informed by Spirit, so is the *Weltseele* the ultimate substratum of physical reality; and the ultimate substratum of physical activity occurs within the fields of force. Hence only spirit can understand the world at its most fundamental, for only spirit can commune with spirit. As Schiller said, "*Was der Geist versprecht, leistet die Natur*". (What the Spirit proposes, Nature disposes). This world-view was held not only by German Romantics but also by cautious natural philosophers in the rest of Europe; and since the first nine chapters of this thesis have been about British figures, let us take an English scientist to illustrate its spread:- On November 4th, 1869, the first issue of the journal *Nature* appeared. Its editor had asked Thomas Henry Huxley to write its opening paper. That paper was crammed with aphorisms by Goethe on Nature; it represented the world-view of one of England's most respected scientists, and it was pure poetry. To chose only two of those aphorisms:

> "Her life is in her children; but where is the mother? She is the only artist; working up the most uniform material into utter opposites; arriving, without a trace of effort, at perfection, at the most exact precison, though always veiled under a certain softness".

> "She has always thought and always thinks; though not as a man, but as Nature. She broods over an all-comprehending idea, which no searching can find out".[51]

If there was an 'all-comprehending idea' over which men 'brooded' during the first few decades of the century, it was the nature of force. In the following chapters we shall examine a handful of such men, all physiologists, all influenced by the charm of *Naturphilosophie*. Occasionally, they seemed not to be affected by it but, as one scholar has asserted in a remarkable paper,[52] even aggresive denunciations of it suggest that it had tainted their early years; and Johannes Müller and Justus Liebig will be particular instances of that.

NOTES TO CHAPTER 10.

1. Bertrand Russell, *History of Western philosophy*, London, Allen & Unwin, 1974.

2. Robert Adamson, *The development of modern philosophy*, Edinburgh and London, William Blackwood and Sons, 1903.

3. Pierre Costabel, *Leibniz and dynamics*, trans. by R.E.W. Maddison, London, Methuen, 1973.

4. J. Kemp, *The philosophy of Kant*, London, O.U.P., 1968.

5. H.J. Vleeschauwer, *The development of Kantian thought*, trans. by A.R.C. Duncan, London, Thomas Nelson & Sons, 1962.

6. C.A. van Peursen, *Body, soul, spirit: a survey of the body-mind problem*, London, O.U.P., 1966.

7. G. Martin, *Kant's metaphysics and theory of science*, trans. by P.G. Lucas, Manchester, Manchester University Press, 1961.

8. I. Kant, *Critique of pure reason*, trans. by N. Kemp Smith, London, Macmillan, 1964. My page references to the *Critique* are given, in the conventional way, to the pages of the first and second editions, which are quoted in Kemp Smith's translation. For instance, A 307 means p.307 of the first edition, and B 209 means p.209 of the second edition.

9. R. Des Cartes, *Discourse on method*, in *A discourse on method; meditations and principles*, trans. by J. Veitch, London, Dent & Sons, 1969, pp.28 and following.

10. *Op.cit.*, Note 2 above, p.60. Also Note 1 above, pp.553 & 554.

11. *Op.cit.*, Note 2 above, p.61.

12. H. Helmholtz, *Treatise of physiological optics*, trans. by J.P. Southall, Wisconsin, G. Banta, 1924, vol.3 esp.

13. *Op.cit.*, Note 1 above, p.563.

14. *Op.cit.*, Note 2 above, pp.68 & 69.

15. *Ibid.*, p.69.

16. For the topic of perpetual motion I have relied greatly on a work that many scholars regard as the *locus classicus* in this area, namely Emile Meyerson's *Identité et realité*, Paris, F. Alcan, 1908. I have used Kate Lowenberg's translation, *Identity and reality*, London, Allen & Unwin, 1930. I have also found useful T.S. Kuhn's 'Energy conservation as an example of simultaneous discovery', and Carl Boyer, 'On the papers of T.S. Kuhn and I Bernard Cohen', in M. Clagett (editor), *Critical problems in the history of science*, Wisconsin, University of Wisconsin Press, 1969, pp.321-356 and 384-390 respectively. Also Yehuda Elkana, *The discovery of the conservation of energy*, London, Hutchinson Educational, 1974.

17. *Ibid.*, Boyer, p.388.

18. For instance, T.S. Kuhn in Clagett, *op.cit.*, Note 16 above, p.334

19. Boyer, *op.cit.*, Note 16 above, p.388.

20. Boyer, *op.cit.*, Note 16 above, p.388.

21. D. Bernoulli, *Hydrodynamica, sive de viribus et motibus fluidorum, commentarii*, Basle, 1738, p.231, cited in Kuhn, p.334.

22. *Op.cit.*, Note 2 above, p.105 briefly.

23. See A.R. Hall, *From Galileo to Newton, 1630-1720*, London, Fontana, 1970, pp.322-324.

24. *Op.cit.*, Note 2 above, pp.79-80.

25. Leibniz, *Philosophische Schriften*, edited by C.J. Gerhardt, Berlin, Weidmann, 1875-90, vol.2, p.72, cited in *ibid.*, p.85.

26. *Ibid.*, p.85. Also Russell, *op.cit.*, Note 1 above, p.565.

27. For instance, J.R. Mayer, 'The motions of organisms and their relations to metabolism', in R.B. Lindsay (editor), *Julius Robert Mayer*, New York, Pergamon Press, 1973, p.90.

28. *Op.cit.*, Note 4 above, p.6.

29. *Ibid.*, p.7.

30. *Ibid.*, p.7.

31. See my Chapter 1. Also J. Locke, *An essay concerning human understanding*, book 2, chapter 21.

32. *Kritik*, *op.cit.*, Note 8 above, A ix.

33. Kemp, *op.cit.*, Note 4 above, p.8.

34. I. Kant, *Prolegomena to any future metaphysic that will be able to present itself as a science*, trans. by P.G. Lucas, Manchester, Manchester University Press, 1953, cited in Kemp, p.9.

35. *Kritik*, A 786.

36. *Ibid.*, Kant's introduction B vii.

37. J. Müller, *Ueber die phantastischen Gesichtserscheinungen*, Coblenz, Jacob Hölscher, 1826, Vorwort, pp.iii-vii. See my Chapter 13.

38. Kemp, *op.cit.*, Note 4 above, p.29 for a very useful summary.

39. *Op.cit.*, Note 16 above, p.338. As any student of *Naturphilosophie* quickly realizes, it is a complex and bewildering field. Indeed, so complex is it that I have not attempted any primary source reading on Schelling, Fichte *et al.*, but have relied on several very discursive and helpful papers. These were as follows: i) B. Gower, 'Speculation in physics: the history and practice of *Naturphilosophie*' in *Stud.Hist.Phil.Sci.*, 1973, 3:301-356. ii) L. Pearce Williams, 'Kant, *Naturphilosophie* and scientific method', in R.N. Giere and R.S. Westfall (editors), *Foundations of scientific method: the nineteenth century*, Bloomington, Indiana University Press, 1974, pp.3-22. iii) E. Mendelsohn, 'The biological sciences in the nineteenth century', *Hist.Sci.*, 1960, 3:39-59.

40. Adamson, *op.cit.*, Note 2 above, pp.253-263. Also Gower in *op.cit.*, Note 39 above. Bertrand Russell does not seem to have thought Fichte important enough to discuss.

41. See particularly Gower, *op.cit.*, Note 39 above, pp.314-315 for a discussion of Schelling's physics.

42. L. Pearce Williams, *op.cit.*, Note 39 above, p.14.

43. *Kritik, op.cit.*, Note 8 above, A 279.

44. L. Pearce Williams, *op.cit.*, Note 39 above, p.15.

45. H.C. Oersted, *Recherches sur l'identité des forces chimiques et electriques, traduit de l'allemand par M. Marcel de Serres*, Paris, J.G. Dentu, 1813. There is a useful and sympathetic article on Oersted by L. Pearce Williams in *Dictionary of scientific biography*, vol.x, New York, Charles Scribner's Sons, 1974, pp.182-186.

46. L. Pearce Williams, *op.cit.*, Note 39 above, p.17. For a lengthier discussion of Faraday's metaphysics, there is L. Pearce Williams, *Michael Faraday, a biography*, London and New York, Chapman & Hall, 1965.

47. H. Helmholtz, 'An autobiographical sketch' in 1891, in R. Kahl, *Selected writings of Hermann von Helmholtz*, Middletown, Connec., Wesleyan University Press, 1971, p.471.

48. For my understanding of 18th century currents of thought I am indebted to the beautifully written work by Basil Willey, *The eighteenth century background*, London, Chatto & Windus, 1940.

49. I regret not discussing von Humboldt's work in any detail in this thesis. He would be ideal for a study of the interactions between philosophy and science, and his influence on his contemporaries was considerable. He seems to have played the part of a fairy-godmother for several young natural philosophers from the 1820s to the 1850s, securing someone a place in a research laboratory here and someone else an academic post there.

To him, Dumas, Liebig and Helmholtz, to mention only three, were indebted for the courses their careers took.

50. Alexander von Humboldt, *Kosmos: a general survey of the physical phenomena of the universe*, trans. by A. Prichard in two volumes, London, H. Baillière, 1845-48. This extract from vol.1, pp.372-373.

51. T.H. Huxley, 'Nature: aphorisms by Goethe', in *Nature*, 1869, 1:9.

52. E. Mendelsohn, 'Explanation in nineteenth century biology', in R.S. Cohen and M.W. Wartofsky (editors), *Boston studies in the philosophy of science*, vol.2, New York, Humanities Press, 1965, pp.127-155.

CHAPTER 11. On power in the physiologies of Johann Friedrich Blumenbach (1752-1840), especially as presented by his English editors, and of Friedrich Tiedemann (1781-1861).

One of those "tough minded and productive scientists" who strongly invoked "vital properties, principles or powers" during the 19th century (to use the words of T.S. Hall in his discussion on vitalism in *Ideas of life and matter*.[1]) was Johann Friedrich Blumenbach. Blumenbach studied medicine at Jena and Göttingen and soon acquired a reputation as a fairly original champion of vitalism and epigenesis. He also acquired a reputation as a teacher for he taught almost two generations of medical students; perhaps his best known student was Alexander von Humboldt. Among his seminal and experimentally-based treatises was his *Institutiones physiologicae* (1786)[2], which was one of the earliest attempts to account for the functions of the body independent of minute anatomical descriptions. It soon became a standard text wherever physiology was taught in Europe, and all four of his Latin editions (Blumenbach preferred writing in Latin) were translated into European languages.

Most historians who have commented on his physiology have not distinguished between successive editions of the *Institutiones*, or discussed the differences in his presentation and interpretation by his different editors. For instance, T.S. Hall has confined himself to the English translation of the third edition by John Elliotson (1817).[3] This does not necessarily give one a false impression of his physiology, but since this thesis has concerned itself so much with British physiology, and since Blumenbach was influential on British (and American) physiology, I shall examine his ideas not only in themselves, but particularly as presented and commented upon by his English editors. I shall examine the 1795 English translation of the first Latin edition by Charles Caldwell,[4] the American physiologist mentioned in Chapter 9, and the 1828 translation of the fourth and last Latin edition by John Elliotson.[5] I shall also examine a lengthy 'Report on animal physiology' read to the British Association for the Advancement of Science in 1834 by William Clark, in order to corroborate my own evaluation of Blumenbach's place in the emergence of dynamical physiology.

The most striking difference between Caldwell's edition and Elliotson's is that Caldwell saw himself merely as a translator of the master's own treatise, whereas Elliotson felt compelled to comment on everything; indeed, Elliotson's own footnotes, appendices etc., were often much longer than Blumenbach's own text and anyone who read that translation must have been liable to lose track of Blumenbach's own ideas. This danger also confronts the historian who relies solely on Elliotson's translation.

Caldwell asserted that, at least in America, there was a widely felt need for a new system of physiology, especially since the condensed English version of Haller's *Institutiones* was so pitiful. For Caldwell, Blumenbach's physiology fulfilled that need. Although Blumenbach was much concerned with the material organization of the body, his physiology was essentially dynamical and focussed on the nature and relations of the vital forces. He enumerated five types of vital power or force[6] (still synonymes generally): simple contractility which, since it seemed to reside in every part of the cellular membranes, he called *vis cellulosa*; the irritability of Haller, which he called *vis muscularis*; sensibility, or *vis nervea*; the specific energies, or *vitae propriae*, which were peculiar to different parts of the body: in this respect he was reminiscent of Bordeu, who gave each organ a life of its own, and anticipated Bichat, who was to regard the tissues, rather than the organs, as the body's functional units; and fifthly, there was the most important of these powers - the *nisus formativus*, or formative tendency, which was responsible for generation. Moreover, since by generation (generatio), Blumenbach had the Aristotelian concept in mind, namely that the processes of nutrition and repair throughout the life of an adult organism are species of generation, this *nisus* was also the efficient cause of nutrition, growth and maintenance; and since his physiology was essentially dynamical, the *nisus* was also the cause or source of the other four types of vital power. Thus, despite his assertion of several vital powers, the urge to reduce Nature to utmost simplicity and to discover a single main-spring in the living mechanism was too tantalizing to avoid. In addition to the *nisus formativus*, this quest for a main-spring can be seen in his ideas on the heart:

> "The heart is, as it were, the first active organ and moving spring of the whole human machine, as it is by the perpetual and truly astonishing energy of this body, that the most important vital function, namely the circulation of the blood, is performed ...."[7]

Like a clock, the living organism consumed its own powers by its own exertions, and in order to be rewound it needed sleep and food; sleep restored the animal powers (*vires animales*), though exactly how, he did not say; food restored the natural powers (*vires naturales*).[8] The organism was therefore not to be considered a *perpetuum mobile*.

Blumenbach was well aware of the complexity and indefinability of the *nisus formativus*, but he believed that its philosophical difficulties were no greater than those entailed in inorganic powers, and that its ontological status was identical to theirs. Therefore, although not saying so explictly, he believed that the discovery of the nature and basic laws of power would benefit equally the physical and the life sciences. In this vein, he wrote

> "... That this energy may not be confounded with the other kinds of vital energy, let it be distinguished by the name of *nisus formativus*. By this name, however, we mean to designate not so much a cause as a perpetual and uniform effect, the existence and reality of which are deduced from actual observations.... It is thus, with views and on principles entirely similar, that we make use of the terms attraction and gravitation, to denote certain energies or sources of action, the causes of which are, notwithstanding, still involved in more than cimmerian darkness".[9]

Indeed, as T.S. Hall has pointed out,[10] he considered his *nisus formativus* doctrine as effecting a bridge between the exclusive study of inorganic powers, (i.e. the iatro-mechanical school), and the exclusively vitalistic approach, (i.e. those who explained development purely teleologically); in this respect, he considered his *nisus* to differ from the *vis plastica* of the ancients and the *vis essentialis* of Wolff, for it posited

> "... *the union and intimate co-exertion of two distinct principles in the evolution of the nature of organized bodies - of the PHYSICOMECHANICAL with the purely TELEOLOGICAL* - principles which have hitherto been adopted but separately by physiologists in framing theories of generation".[11]

Caldwell did not comment much on Blumenbach's text; his overall reaction came in 'An appendix on animal electrity' which he had written and bound with the main treatise. There he eulogized Blumenbach as the philosopher who had brought Haller's physiology most up to date; Caldwell also believed that the greatest development in recent physiology had been the discovery of animal electricity, and he gave highest praise to "the youthful Mr. /Richard/ Fowler" for his researches.[12]

In his own career, Caldwell does not seem to have championed the *nisus formativus* doctrine, but he did maintain a belief in the utmost importance of the early formative processes in human beings; this resulted in his being deeply interested in education, which he explicated according to his own theory of vital force. Caldwell seems to have abandoned Blumenbach's belief in several vital forces, and in so doing he might have been influenced by another of Blumenbach's English translators, namely, Alexander Crichton (1763-1856), who in his 1792 translation of Blumenbach's *Über den Bildungstrieb* (first published 1781)[13] hinted that the *nisus formativus* was only a manifestation of an all-encompasing vital principle. Caldwell's belief in a single vital principle and the importance of the early formative stage in an organism's development can be seen from the following extract, taken from his *Thoughts on physical education and the true mode of improving the condition of Man* (1844):-

> "In fact, physical education, hitherto so much neglected, and still so imperfectly understood and practised, may be pronounced the ARBITER of the human mind, no less than of the human body. Its influence in strengthening and weakening, improving or deteriorating, all kinds of mental faculties and operations, is far greater than is commonly supposed. Through its instrumentality alone can man attain, in mind as well as body, the highest perfection of which he is susceptible. It is destined, therefore, ... to be the chief agent in the production of the millenium, at whatever period that improved condition of our race may occur".[14]

When he wrote this, Caldwell was a well-established academic figure in America, and his physiological - and phrenological - writings were being read widely.

The third and fourth editions of Blumenbach's treatise were translated by John Elliotson, a prominent teacher of medicine in London, a founder of University College Hospital[15] and founder and first President of the Phrenological Society. The fourth edition had been retitled *Primae lineae physiologicae*; Elliotson's translation was especially useful to his contemporaries (and is equally useful to historians), for Blumenbach made many more references to his colleagues' works than he had in the first edition, and his editor added a wealth of his own references; for instance, Elliotson was widely read on the German contribution to vital force and gave his readers a very solid reading list.[16] Blumenbach himself referred to several

English physiological studies which have been mentioned earlier in this thesis; for instance, on "what the English have lately termed specific irritability", that is, the different reactions of different parts of the body to the same stimulus, he cited Samuel Farr's *On animal motion* (1771) and Gilbert Blane's *On muscular motion* (1788);[17] on the dependence of health upon the number, as well as upon the energies, of the bodily functions, he praised Sir Gilbert Blane's *Elements of medical logic*: "The very acute Gilbert Blane's classification of the functions of the animal economy according to the powers which direct them, surpasses all other modern attempts of the kind".[18]

Perhaps Blumenbach's most obvious mentor in the fourth edition was Galen.[19] For instance, he adhered by and large with the division of living functions into four faculties - the vital, animal, natural and genital - abiding by this division when discussing which components of the animal body were truly alive. Thus, he denied that the blood was alive, although he attributed life to the genital fluids,[20] for they evidently possessed a faculty unique to themselves and essential to life; Elliotson disagreed with him on this point, quoting a venerable list of authorities for the life of the blood - Aristotle, Harvey (who had attributed the idea to Moses), Francis Glisson (1597-1677), Bernard Albinus (1697-1770), and John Hunter (1728-1793); however, Elliotson also realized how intricate the issue was and called Dugald Stewart to his aid:-

> "The essential nature of life is an impenetrable mystery and no more a subject for philosophical inquiry than the essential nature of attraction or of heat. To attempt explaining the phenomena of life by a vital fluid is only increasing the intricacy of the subject by an unfounded *hypothesis*, and always reminds me of Mr. Dugald Stewart's remark - 'That there is even some reason for doubting, from the crude speculations on medical and chemical subjects which are daily offered to the public, whether it (the proper mode of studying nature) be yet understood so completely as is commonly imagined, and whether a fuller illustration of the rules of philosophizing, than Bacon or his followers have given, might not be useful even to physical inquirers'".[21]

Elliotson seems to have been well aware of the recent, and often sophisticated, theories on forces that had been developed in natural philosophy and physiology, and he considered it his duty to bring Blumenbach's otherwise sound text up-to-date on those issues; for instance, Blumenbach's own text on 'Respiration and its principal use'[22] occupied only seven pages (including footnotes) in Elliotson's edition, and he offered a material, non-dynamical, view of it, taking his cue solely from Harvey who had suggested that "the use of expiration is to purify and ventilate the blood, by separating from it these noxious fuliginous vapours".[23] By contrast, Elliotson discussed William Prout's galvanic theory of respiration, whereby more than mere chemical changes were supposed to be effected, namely the production of galvanic force.[24] He made a dynamical addition to Blumenbach's discussion of animal heat, concluding on the cautionary note that animal heat, though usually discussed as if it was corpuscular, was probably only a state in which matter exists, as were ordinary heat, light and electricity; indeed, he suggested that they were all modifications of the same state.[25]

Elliotson believed that the elucidation of the natures and essential unity of all dynamical agents in natural philosophy would benefit physiology too, for it faced similar problems in discussing agents like nervous power;[26] such physiological agents had, like the physical ones, been explained by an oscillation hypothesis, and although that was ingenious, Elliotson did not think highly of it as a representation of the reality of life:

> "We might as well attempt to explain the phenomena of motion or of chemical affinity and galvanism by vatality and mind, as the phenomena of vitality and mind by mechanics or chemical affinity or galvanism. They are altogether distinct principles, although there can be no question that the laws of mechanics and chemical affinity and galvanism are important and indispensable in every living system, in subservience to life and mind. The mind, for aught we know, may stimulate the voluntary muscles by means of galvanism communicated along the nerves, but then the galvanism is not mind, it is merely an instrument employed by the mind".[27]

The relations between inorganic and organic powers were developed further in his additions to Blumenbach's text on secretion: the vital powers could be said to bring into play the purely chemical relations between the particles which constitute the living organism; exactly how this happens was enigmatic, but of one fact he was sure - that life cannot create or annul the physical and chemical qualities of matter, that it may only counteract one inanimate force by another.[28]

Clearly, Elliotson envisaged a rigorously law-abiding scheme for organic and inorganic powers alike, though the precise mechanism was yet inscrutable.

In discussing sexual indulgence, Blumenbach adhered to the idea that it deprived the animal economy of much of its power; thus, ejaculation of semen was described as "... a succussion of the whole system, short and less violent though of an epileptic nature, and followed by a depression of strength".[29] Elliotson added a lengthy historical comment to this passage, mentioning that, for this reason, Zeno the stoic, had called the loss of semen the loss of animating principle and that he had therefore "embraced his wife but once in his life, and then out of mere politeness".[30] The atomists, Epicurus and Democritus, were of the same opinion, and the *athletae* never married, lest their strength be impaired. Moreover, the Jewish rabbies, in their anxiety to preserve their nation, were said to have ordered, with a view of preventing loss of vigour, that a peasant should indulge but once a week, a merchant but once a month, a sailor but twice a year, and a studious man but once in two years. Moses forbade indulgence before a battle. Evidence from plant physiology was adduced - for instance, that many plants die soon after they have flowered, and the removal of the sexual apparatus from plants often renders annual ones biennial, and biennial ones triennial.[31]

Elliotson did not support the theory, which was still popular, that the ardour with which a procreating couple embraced affected the energy of the offspring;[32] yet he admitted there was some evidence for that idea, since it had been remarked that bastards were frequently endowed with great genius and valour, for which history gave many examples; the phenomenon was generally ascribed to the impetuousity of the parents during their embraces, for which one could cite no less a student of human nature than Shakespeare himself: in King Lear, Edmund bursts into indignant soliloquy:

> "Why bastard? Wherefore base?
> When my dimensions are as well compact,
> My mind as generous, and my shape as true,
> As honest madam's issue? Why brand they us
> With base? with baseness? bastardy? base? base?
> Who in lusty stealth of nature
> Take more composition and fierce quality
> Than doth, within a dull, stale, tired bed
> Go to creating a whole tribe of fops
> Got between sleep and wake?" (Act 1, Sc.2).[33]

Edmund was in good company, for other bastards of great attainments were Hercules, Romulus, Alexander the Great, King Arthur, William the Conqueror, Homer, Pope Adrian the fourth and Peter Lombard. Such historical and literary allusions were not at all out of place in early - mid 19th century scientific treatises, for scholars in all fields were often widely read in the classics and other literature (and were occasionally still writing their treatises in Latin in the 1830s and 1840s).[34] It was therefore useful for Blumenbach, Elliotson and others to be able to cite historical examples in support of their ideas on longevity, sexual indulgence and vital power. If we try to extract the basic 'scientific' ideas that such citations were intended to illustrate, there are at least three worth mentioning: 1) That the power of the vital economy was never *sui generis*, but depended ultimately upon a supply from without; the exact source of this supply was not always discussed, although we shall see that Tiedemann discussed it explictly; 2) That, as with inorganic mechanisms, a primary concern in the vital economy was the economy of power; and 3) that in the process of procreation, vital power played the principal rôle of being the vehicle for the transmission of parental characteristics and of life itself.

Another view of what Blumenbach's ideas on vital forces, particularly the *nisus formativus*, meant to British physiologists can be got from *A review of the doctrine of a vital principle* (1829),[35] by James Prichard, a physician and Fellow of the Royal Society. Prichard refuted the doctrine of a vital force as a real entity in the vital economy, although he did not doubt the unique phenomenal character of life itself; his own view was that the vital functions, including the circulation of the blood, respiration and its consequent production of animal heat, were evidently contrived and arranged with wonderful art, but were essentially mechanical; and that there was a variety of chemical relations between living and inorganic matter, whereby actions and reactions occurred continually and thus supported the vital activities of the animal economy. To mechanism and chemical affinity he believed all the phenomena of animal life would be attributed eventually.[36] Clearly, the vital system was not to be envisaged as a *perpetuum mobile*; on this point he critized the physiologists who had come before John Hunter's new mode of theorizing, for instance, "the celebrated Dr. Mead", who had compared the living body to a machine endowed with the property of perpetual motion. Mead had said that God alone could complete such a machine, and God was pleased that the body should be a fabric of that sort, by disposing all its parts in such a manner, that they should form a kind of circle in which, at the same time that they performed their respective functions, they should constantly and mutually repair each other.[37] Since Mead's time, Prichard believed that physiology had come a long way and that there had lately been two major views of life that had deservedly been attracting much attention; one was the doctrine of the *Bildungstrieb* or *nisus formativus*,

> "... which originated with the venerable Blumenbach, and which has been adopted recently, together with the terms connected with it, by some of the most distinguished naturalists in France".[38]

According to Prichard, it was commonly felt that Blumenbach's theory was rather obscure and difficult to unravel; he had therefore decided to present a translated abstract of Blumenbach's original essay, '*Uber den Bildungstrieb (Nisus Formativus), und seinem Einfluss auf die Generation und Reproduction*' from the Göttingen Magazine of 1781.[39] Among the extracts that he quoted from the paper was one which suggested a single, basic, vital force:

> "'A truth', says the author, 'which must never be lost sight of in these inquiries, and the neglect of which may often have impeded their successful prosecution, is this, that generation, nutrition and reproduction, are fundamentally mere modifications of the same energy, which in the first instance constructs, in another maintains, and in a third repairs; in other words, nutrition is an universal but imperceptibly

continued reproduction, and the latter a repeated but only partial generation'".[40]

Yet in his paper, Blumenbach had emphasized that the *Bildungstrieb* was not to be confounded with the *vis plastica* (as defined by Francis Bonamico, the Aristotelean, in his *De Formatione Foetus*) or the *vis essentialis* (of Caspar Wolff), both of which had been propounded as a fundamental vital force with which all other vital forces could be correlated.[41] The difficulty of interpreting what Blumenbach meant might be alleviated if we suppose that he was adhering, by and large, to Aristotle's idea that all processes of nutrition, growth and repair in organisms are essentially analogous to generation, for they could all be envisaged as processes of 'coming-to-be'. As such, they had a common character and could be considered particular modes of a single process, energy or *nisus*. Like Aristotle,[42] Blumenbach considered generation to be the key to physiology, for that is the point at which the materials, powers and form of the creature come into being; all subsequent processes in the creature's life are essentially means of maintaining what had already come to be.

Blumenbach's *Bildungstrieb* made a strong impression on his contemporaries for it offered a new argument for epigenesis against preformationism. Moreover, his entire physiology seemed to promise a new viewpoint, as we see in the British Association Report on physiology, mentioned earlier. The author of that Report, William Clark, was a physician, F.R.S., and professor of anatomy at Cambridge. He traced the development of physiology pretty accurately, and even when discussing the work done in the past fifty years he maintained a remarkable level of objectivity even though it became clear what his own theoretical position was. (In fact, his *Report* is most valuable for an historian requiring an overall view of physiology throughout the 18th and early 19th century and is far more useful than most modern histories of the subject). In the *Report*, Blumenbach figured as one of the earliest dynamical physiologists in his insistence on every part of an organism having its own characteristic degrees of excitability and nervous power, and hence its own mode of life. Clark put him alongside the French vitalists, Théophile de Bordeu (1722-1776) and his pupil Paul Barthez (1734-1806), for developing this opinion roughly simultaneously and for also admitting - tentatively on Blumenbach's part - a fundamental power, called *vis vitae*, of which the different degrees of excitability and sensibility could be considered mere modes. So far so good, but for Clark and for some of Blumenbach's German contemporaries there was a serious flaw since the means whereby the basic *vis vitae* becomes modified had not been explained:

> "It might have been foreseen that this analytical mode of treating the living organism - this isolation of powers which had been intended by their concurrent acts to produce the phenomena of life - could scarcely lead to the detection of that controlling cause which forced the whole to conspire to a common purpose".[43]

In other words, for Clark and others, a dynamical physiology was admirable but it had to have a teleological focus; with this we come to a new group of physiologists of whom the most eminent, in Clark's opinion, was Friedrich Tiedemann.

On Power in the physiology of Friedrich Tiedemann (1781-1861)

Friedrich Tiedemann graduated M.D. at Marburg University in 1804, and in the next half dozen years came under the influence of three men who were to shape his career as a thorough experimentalist. Firstly, he stayed on at Marburg to attend courses in physiology, osteology and cranioscopy by Franz Joseph Gall (1758-1828), whose skill in anatomical preparations set him a standard for his own later work. He then went to Würzburg to study with Schelling; he managed largely to resist the temptations of *Naturphilosophie*, and according to his own admission, Schelling was responsible for his decision to pursue empirical research.[44] The third influence was that of Georges Cuvier (1769-1832), under whom he studied in Paris and from whom he acquired his appreciation of comparative studies.

Tiedemann was a teacher of anatomy and physiology for over forty years, during which his literary output included some remarkable and seminal works. Perhaps the most remarkable was the study of digestion that he did with the chemist, Leopold Gmelin (1788-1853), which was published in 1826;[45] its innovating character can be seen from the praise given to it by the great Jöns Jacob Berzelius (1779-1848), as "uncontestably the most complete physiological examination, which has enriched the chemical study of the processes that occur in living animals".

Most of Tiedemann's major works were translated widely; his most oft-cited work in Britain was the *Physiologie des Menschen* (first published in 1830) which, as can be seen from Clark 's British Association Report exerted some influence on British physiologists even before its translation; after translation, it became one of the oracles in British physiology - a status it had acquired already in Germany. In this treatise we find his fullest discussion of power in physiology.

Tiedemann was a fully committed vitalist and saw himself in the school of thought of Reil and Hufeland. Many of his central ideas on vital force have therefore been mentioned in the preceding chapters, and here only his own contributions to the physiological use of power will be discussed. He developed the dynamical explanation of generation that had been espoused by Hufeland, calling the agent responsible for it the power of formation (*Bildungskraft*). This was the most fundamental power in all living systems for, as Blumenbach had argued, it was the agent that synthesized the organism and its powers from the moment of conception. The formative power was responsible not only for the generation of new individuals, but also contributed to the maintenance of their vital powers throughout life; on this point he quoted the 17th century physician, George Ent (1604-1689), who had reasserted the Aristotelian idea that nutrition resembles a continued act of generation in each living organism, and that the organism is a continuous balance of coming-to-be and passing-away.[47] Tiedemann realized that only a dynamic theory of generation could resolve the challenge to the generative theories of Harvey and Linnaeus that had been posed by the more recent researches of John Turberville Needham (1713-1781), Joseph Priestley (1733-1804), Heinrich August Wrisberg (1739-1808), Otto Frederik Müller (1730-1784), Gottfried Reinhold Treviranus (1776-1837) and others, who had shown that certain simple animals and vegetables could be formed without the concurrence of living organisms.[48] The explanation was wholly dynamical and elegant:

> "The power of formation which calls organic bodies into existence in generation, which produces all their tissues and parts, together with their vital properties, in the germinative fluid, which develops, completes and maintains them during their life, should be considered as the primitive and fundamental power of these bodies, as the creator and preserver of all the powers that belong to living bodies, either in their ensemble or in their different parts. Hence, physiologists and physicians who seek the principle of life in any other power than it, in excitability, irritability, or sensibility, commit a great error ...."[49]

Actually, there was a more fundamental power than that of formation, namely, the plastic power (*vis plastica*). As its name implies, it was responsible for the initial plasticity or formation of organic beings and their powers. Tiedemann grappled with the question of the source of the plastic power itself, for he seems to have been reluctant to allow that any power could be *sui generis*; however, he was forced to admit that, so far as science was concerned, this question was and always would be insoluble, and that physiologists would have to treat it as a primary irreducible agent. Not that he was content with this admission, for it was an admission that the living organism might be a *perpetuum mobile* and therefore forever inscrutable. The only respectable way out of tne dilemma was to derive the plastic power from God, which, like Hufeland, Tiedemann did. However, whereas Hufeland's God as the source of all power was an intrinsic and frequently invoked component of his physiology, Tiedemann's God seemed to be only a desperate solution to an intractable physiological problem. This is not to say that Tiedemann was insincere in his employment of the Deity; it simply means that he was reluctant to resort hastily to God to solve scientific problems. Here we see how well he had escaped the enchantment of *Naturphilosophie*, and how Hufeland had not. Hufeland could never admit, as Tiedemann did, that the questions of primary and final causes were utterly beyond human understanding:

> "It if be asked, whence organic matters proceed, how they are produced, together with the power of formation inherent in them, we are necessitated candidly to confess our ignorance on the subject, inasmuch as the first origin of organic matters and living bodies is altogether beyond the range of experiment, as is also that of inorganic bodies and matter in general. The final cause of the existence of the plastic power is, like that of all the other powers, of attraction, of repulsion and their modifications, mechanical attraction, gravitation, cohesion, and adhesion, as also that of chemical affinity, a secret whose profundity, as Buffon said, we shall never be able, from all appearances, to reach ...."[50]

Yet the nagging question of the origin or source of vital power, of whether it could be created *ab nihilo*, was again apparent in his discussion of muscular contractility and nervous power. The debate between those who maintained that muscle possesses its own inherent power for contraction, (the school of Haller), and those who derived muscle power from nerve power (Robert Whytt,[51] Alexander Monro *secundus* (1733-1817) and their followers), was therefore partially envisaged as a question of whether vital forces could be *sui generis*; of course, it was a question of other things too, for instance, whether any single vital force could operate in total isolation from all other vital forces. Tiedemann felt obliged to emphasize that the very concept of organism entailed a mutual connection among all its powers and functions.[52] His view of muscle power was a compromise between the two main schools; he agreed with Haller that there is a special force inherent in muscular fibres, but Haller had erred in not asking what maintained that force.[53] In similar vein, he praised the doctrine of John Brown but criticized its neglect of the origin of organic powers and the principles concerning their exhaustion and renovation:

> "Brown left altogether untouched the question, what passes in the organs during the action of external stimuli, and wherefore excitation diminishes, and finally even annihilates, excitability. The reaction of living parts, and thier excitability, are diminished by the operation of excitants, but rest re-establishes them. How this happens, he did not attempt to inquire, or explain. In short, he paid no attention to the internal condition necessary for the maintenance and preservation of excitability".[54]

Tiedemann's answer to the question that he thought Haller and Brown had neglected was to envisage nutrition primarily as a dynamical process, rather than as a material phenomenon which was the commoner point of view, and to attribute to nutrition the renovation of the organism's power. Indeed, he offered perhaps the boldest dynamical theory of nutrition before those of Müller and Liebig. He asserted that:

> "During sleep the necessary conditions of this material constitution are renewed by the operation of nutrition, as also the external and internal excitants that maintain them. Hence, the forces of these parts have their energy renovated, and recover the power of acting under the influence of excitants that urge them to activity. It is the force of nutrition, therefore, which not only originates excitability and activity of the nerves, sensitive organs and muscles, but also renovates them when they are exhausted by exertion. Similar phenomena of a varied state of excitability and mobility, according to the diurnal periods and the kinds of excitants, are observed in the leaves, flowers and genitals of plants, in which they are also only an index of the changes they undergo in the state of their nutrition according to the periods of the day and the excitants they meet overall".[55]

In his section on nutrition and its relation to muscle power he asserted his kinship with Reil and Hufeland on what they meant by excitability. However, he stressed that excitability was not an isolated force, wholly independent of the organic matter to which it was attached, but that it was associated inextricably with that matter. He seems to have misunderstood Reil, for if one realizes the purely heuristic status of Reil's vital force, his ideas on the relation between living forces and organic matter were almost identical to Tiedemann's.

Vital forces were not the only forces in the organism that Tiedemann discussed; the evolution of the imponderables - heat, light and electricity - took up about 60 of the 430 pages of the book. Although well aware of the debates on the natures of these imponderable agents, he refrained from discussing whether they were true powers or imponderable substances. What was important about them, for physiology, was solely that they seemed to result from the material changes connected with the nutritive functions,[56] and their phenomena were therefore central to the animal economy. (Interestingly, in discussing animal electricity, the only British researcher he mentioned was Richard Fowler; indeed, Fowler's book on animal electricity (1793) was cited several times in the treatise.[57]) In this and other sections, he suggested that the elucidation of the primary sources of the commonly mentioned forces of Nature would be a great step forward in both physiology and physics, and that this investigation into the nature of force might be mounted on two fronts - the physiological and the physico-chemical. The same philosophical and technical problems confronted the question of force in physiology, physics and chemistry, and like Blumenbach he looked forward to the day when power or force would be explicated in a grand, overall theory and would be buried no longer in ambiguity.[58]

Tiedemann's general physiology is rarely discussed by historians, for his work with Gmelin on digestion has stolen the limelight. Nevertheless, on his contemporaries his dynamical physiology exerted considerable influence; and that it influenced British physiologists can be seen from the frequency with which they cited him during the 1830s and 1840s and from William Clark's *Report*. In Clark's summary of his own theory of embryological development we can see the influence of Tiedemann, for instance, the idea that the nutritive substance, the yolk, which occurs in the proximity of the germ at the beginning of the germ's evolution, is destined primarily to enable the germ to work;[59] nutrition is the provider of energy. Also in his ideas on the different forms of energy in the organism being manifestations of a single fundamental force, Clark was in line with him.

Indeed, Clark called the *Physiologie* an "excellent work, worthy of the great name of its author"[60] and implied that its influence on British physiology was profound.

Finally, a comment on Tiedemann's metaphsics. There is very little explicit indication in any of his physiological writings of any important interaction between his metaphysics (whatever they were) and his physiology. He wrote of 'conflict' between the forces of the organism and external forces, and used other phrases reminiscent of *Naturphilosophie*, but nowhere was there any explicit acknowlegement of a philosophical background. Perhaps further study of his non-physiological works will reveal something, but in the meanwhile one has to concur with his biographer, Theodor Bischoff,[61] that having resisted the lure of *Naturphilosophie* at Würzburg as a young man, he remained true to empiricism.

NOTES TO CHAPTER 11.

1. T.S. Hall, *Ideas of life and matter*, Chicago, Chicago University Press, 1969, vol.2, p.99.

2. J.F. Blumenbach, *Institutiones physiologicae*, Göttingen, J.C. Dieterich, 1786.

3. *Op.cit.*, Note 1 above, p.100.

4. J.F. Blumenbach, *Elements of physiology* ..., translated from the original Latin, and interspersed with occasional notes. By Charles Caldwell. To which is subjoined, by the translator, an appendix exhibiting a ... view of the existing discoveries relative to the subject of animal electricity. 2 vols in 1. Philadelphia, T. Dobson, 1795.

5. J.F. Blumenbach, *The elements of physiology*, translated from the fourth Latin edition by J. Elliotson, London, Longman, 1828.

6. *Op.cit.*, Note 4 above, pp.32-35.

7. *Ibid.*, p.66.

8. *Ibid.*, vol.2, p.1.

9. *Ibid.*, vol.2, pp.176-177.

10. *Op.cit.*, Note 1 above, pp.104-106.

11. Cited in *idid.*, p.104.

12. *Op.cit.*, Note 4 above, pp.228-247 for a discussion of Fowler's work.

13. J.F. Blumenbach, *An essay on generation*, trans. by A. Crichton, London, T. Cadell, 1792 has been the edition I have used.

14. C. Caldwell, *Thoughts on physical education and the true mode of improving the condition of man*, Boston, Marsh, Capen & Lyon, 1834, p.24.

15. See Leslie Stephen (editor), *Dictionary of national biography*, London, Smith, Elder & Co., 1908, 6:683. Also G. Rosen, 'John Elliotson, physician and hyponotist', *Bull.Inst.Hist.Med.*, 1936, 4:600-603.

16. *Op.cit.*, Note 5 above, p.27.

17. *Ibid.*, p.34.

18. *Ibid.*, p.52.

19. *Ibid.*, chapter 'On health and human nature', pp.50-53.

20. See Elliotson's comments on Blumenbach's ideas in *ibid.*, p.62.

21. Dugald Stewart, *Elements of the philosophy of the human mind*, vol.1, p.8. cited in *ibid.*, p.64.

22. *Ibid.*, pp.110-116.

23. *Ibid.*, p.116 for Blumenbach's position; and p.138 for Elliotson's comment and quotation from Harvey.

24. *Ibid.*, pp.133-134.

25. *Ibid.*, p.171.

26. *Ibid.*, pp.198-199.

27. *Ibid.*, p.226.

28. *Ibid.*, pp.399-400.

29. *Ibid.*, p.440.

30. *Ibid.*, p.451.

31. *Ibid.*, p.451.

32. *Ibid.*, p.495.

33. *Ibid.*, p.496.

34. This is true not only of German and other continental writers, but also of British ones occasionally. See, for instance, Thomas Hodgkin's '*Dissertatio physiologica inauguralis de absorbendi functione*', in W.F. Edwards, *On the influence of physical agents on life*, London, S. Highley, 1832, pp.343-381.

35. J.C. Prichard, *A review of the doctrine of a vital principle*, London, Sherwood, Gilbert & Piper, 1829.

36. *Ibid.*, pp.113-114.

37. *Ibid.*, p.211.

38. *Ibid.*, p.213.

39. *Ibid.*, pp.213-224. See also T.S.Hall, *op.cit.*, Note 1 above, pp.102-105.

40. *Ibid.*, p.216.

41. *Ibid.*, Prichard's footnote to pp.215 & 216.

42. Aristotle, '*De generatione et corruptione*', in W.D. Ross (editor), *The works of Aristotle*, vol.2, Oxford, Clarendon Press, 1930. Also '*De generatione animalium*', in J.A. Smith & W.D. Ross (editors), *The works of Aristotle*, vol.5, Oxford, Clarendon Press, 1912,

esp. part 2, bk.2 for generation and nutrition, and part 3, bk.4 for regeneration.

43. W. Clark, 'Report on animal physiology; comprising a review of the progress and present state of theory, and of our information respecting the blood and the powers which circulate it', in *Report of the fourth meeting of the British Association for the Advancement of Science*, London, J. Murray, 1835, pp.95-142. This quotation, pp.105-106.

44. V. Kruta, 'Friedrich Tiedemann' in *Dictionary of scientific biography*, New York, Charles Scribners' Sons, 1975, vol.13, pp.402-404.

45. L. Gmelin and F. Tiedemann, *Verdauung nach Versuchung*, 2 vols., Heidelberg, K. Groos, 1826-27. Discussed in detail in *ibid.*; in J.S. Fruton, *Molecules and life*, New York, John Wiley, 1972, esp. pp.68-71; and in H.M. Leicester, *Development of biochemical concepts*, Cambridge Mass., Harvard University Press, 1974, chapter 14.

46. Cited in *op.cit.*, Note 44 above, p.403.

47. F. Tiedemann, *A systematic treatise on comparative physiology, introductory to the study of man*, trans. by J.M. Gulley & J.H. Lane, London, J. Churchill & T. Kaye, vol.1, 1834, p.179.

48. *Ibid.*, p.39.

49. *Ibid.*, p.188.

50. *Ibid.*, p.193.

51. There is a succinct and still useful discussion on Whytt in J.D. Comrie, *History of Scottish medicine*, London, The Wellcome Historical Medical Museum, 1932, vol.1, pp.307-308.

52. *Op.cit.*, Note 47 above, p.295.

53. *Ibid.*, p.295.

54. *Ibid.*, p.372.

55. *Ibid.*, p.388.

56. *Ibid.*, English edition, p.229.

57. *Ibid.*, particularly in chapter 3 of the section 'On the evolution of imponderables'; for instance, pp.292-293.

58. *Ibid.*, pp.392-393.

59. Clark, *op.cit.*, Note 43 above, p.106.

60. *Ibid.*, p.108.

61. Theodor Bischoff, *Gedächtnissrede auf Friedrich Tiedemann*, München, Verlag der Königliche Akademie, 1861.

CHAPTER 12. On power in the physiology of Christoph Wilhelm Hufeland (1762-1836)

Hufeland was cited often by German physiologists during the first half of the 19^{th} century and not infrequently by British authors. It is therefore curious that very scanty attention has been paid him by historians of medicine in the English speaking world. The *Dictionary of scientific biography* does not mention him and, to my knowledge, there is not a single rounded appreciation of his life and work in English - not even a paper in a history of medicine journal.[1] By contrast, he has been studied in depth by German, Czech,Polish and Russian scholars who have clearly appreciated his stature. On the German secondary literature,[2] as well as on his own writings, this chapter relies.

At the age of eighteen Hufeland began his medical studies at the University of Jena. However, finding that the atmosphere there was uncongenial to serious study he moved to Göttingen where he came under such eminent teachers as August Gottlob Richter (1742-1812), Ernst Gottfried Baldinger (1738-1804), Blumenbach and Gmelin. In 1783 he graduated with a thesis '*De usu vis electricae in Asphyxia*' (The use of electricity in the treatment of asphyxia). He immediately returned to his home town of Weimar and took over his father's medical practice. To begin practice so young, without having spent a few years travelling and gaining different experiences in medical theory and practice, was fairly unusual, particularly if one had any ambition. However, Hufeland's career did not suffer from his ten years' confinement to Weimar, for that city was then a hubbub of intellectual activity. A group of scholars had assembled there around the magnetic personality of Goethe. Hufeland became their personal physician, was a regular attender at Goethe's *Freitagsgesellschaft* and became well acquainted with men like Johann Gottfried Herder (1744-1803) the philosopher and historian, Christoph Martin Wieland (1733-1813) the poet, and Friedrich Justus Bertuch (1747-1822) the jurist. At one of Goethe's friday-evening gatherings Hufeland was reading from his notes that he had been compiling during his medical practice, (these notes were published in 1796 as his *Makrobiotik*), and so impressed the Archduke and Goethe that it was felt he deserved a chair at Jena. The following spring (1793) he began his professorial duties.

His spell at Jena brought him into contact with another galaxy of scholars - the philosopher Johann Gottlieb Fichte, the poet and historian Friedrich von Schiller (1759-1805) and, most importantly for his medical work, the fine anatomist and physiologist Johann Christian Reil. Inevitably he came into close contact with *Naturphilosophie*; yet, largely because he had spent the past ten years in medical practice, which had often been gruelling and had left him with little time to develop a too speculative turn of mind, he managed to avoid excessive importation of metaphysics into his medical writings and teaching. Throughout his life he genuinely considered his obligations to his patients to far outweigh obedience to any system, and although he was ready to theorize in physiology, his theorizing was always close to the ground of empiricism. As one historian recently wrote:

> "*Wann Systematiker wie BROWN, HAHNEMANN, MESMER oder die Naturphilosophen versuchten, alle Lebensvorgänge aus einem einheitlichen Prinzip zu erklären und sich dabei weitgehend theoretischen Spekulationen überliessen, so betonte HUFELAND demgegenüber den Wert der Vernunft und der Erfahrung in der Medizin. Ohne sorgfältige Beobachtung am Krankenbett, ohne Experimente und kritisches Nachdenken und ohne ein gründliches Studium der Literatur kann der Arzt nicht auskommen*".[3]

Indeed, Hufeland's loyalty to his patients and the many words he therefore wrote on the ethics of medical practice have resulted in his being remembered chiefly as "an ideal doctor, a moralist, a philosopher and a gentleman",[4] rather than as a physiologist of some originality. One 20th century medical historian has gone so far as to call him "one of the great philanthropic physicians"[5] of all time, and with this it would be difficult to disagree. But we must not lose sight of his contributions to academic medicine. He edited four journals during his life, the most impressive being *Hufeland's Journal*, alias *Journal der praktischen Arzneikunde*, which ran to eighty-two volumes between 1795 and 1836. In 1796 he published the *Makrobiotik: Die Kunst, das menschliche Leben zu verlängern*,[6] and between then and his death in 1836 published a not inconsiderable number of papers and treatises, some of which were largely concerned with purely practical matters whilst others were largely concerned with theoretical issues. Of the latter kind we might mention his papers on animal magnetism[7] and homoeopathy,[8] for they were especially influential.

His last work to be published, the *Enchiridion Medicum, oder Anleitung zur medizinischen Praxis* (1836),[9] tells us by its very title that its aim was practical rather than theoretical. Nonetheless, it contained some highly revealing passages on his physiology, a physiology which seems to have been essentially dynamical throughout his life. To examine that physiology, we shall discuss two of his treatises, the *Makrobiotik* and the *Enchiridion*, keeping our eye on some of his other works too.

Before doing that, however, a few more biographical details ought to be mentioned, both because they are rarely known among English-speaking medical historians and to emphasize that we are not dealing with an insignificant figure in medical history. In 1801 he was called to Berlin as director of the Collegium Medico-Chirurgicum and principal physician at the Charité Hospital. From 1806 to 1809 he served as personal physician to the Royal Family of Prussia during exile, and on its return played a leading rôle in the reorganization of the country's medical services.[10] In 1810 he was appointed to the chair of special pathology and therapy in the new University of Berlin and later he founded the medical-chirurgical society which became officially named the Hufelandsche Gesellschaft (1833). His last years were spent in ordinary medical practice.

Hufeland's physiology had strong affinities to those of Cullen, Brown, Blumenbach and Tiedemann. It was based on the idea that the living organism possesses a determinate capacity for reacting to stimuli. In addition, he was a fully committed vitalist and was intent on exploring the

the nature and laws of *Lebenskraft*, which he did with considerable reliance on empirical and experimental evidence (although he himself could not be called an experimentalist), and in this respect he ranks beside Johann Christian Reil as a systematic exponent of vital force. Unfortunately, most historians of the German enunciation of vital force have largely, and often totally, ignored Hufeland and have focussed on Reil: for instance, Mikulâs Teich in his paper 'On the historical foundations of modern biochemistry' (1970),[11] discussed only Reil's contribution to the vital force concept among the late 18th century Germans; Teich is too fine a scholar not to have realized that there were other important workers in the field, and in fairness to him, he seems to have singled Reil out simply as "a very fine example of how to blend experimental knowledge with theoretical thought"[12] in the working-out of a concept like vital force. Marcel Florkin in his *A history of biochemistry* (1973),[13] Part 1, (1972), mentioned Reil as the principal critical student of vital force and also mentioned - but only mentioned - that Hufeland contributed too. Noel Coley in his *From animal chemistry to biochemistry* (1973)[14] mentioned only Reil's work, saying that "The first systematic treatment of the concept of vital force appeared in the opening paper of the first issue of *Archiv für Physiologie*, published by J.C. Reil in 1795".[15] Considering that Hufeland openly elaborated his ideas on vital force in the early 1790s, those ideas being published in his *Makrobiotik* in 1794, it is historically inaccurate to give all or most of the credit to Reil, and it seems to me that much more research on primary sources needs to be done on this issue.

Although I have not had sufficient time to undertake this task, one purpose of this chapter is to show briefly that Hufeland was an important contributor to the vital force doctrine during the late 18th and early 19th centuries.

In the preface to the *Makrobiotik* he declared his wholly dynamical approach to physiology, one branch of which was the macrobiotic science of prolonging life. This differed from ordinary medicine in not attempting to maintain a person in maximum fitness and vitality, but seeking rather to make his vitality last as long as possible at a level somewhat below maximum possible intensity.[16] Macrobiotic science dealt with man's moral and spiritual nature in addition to his bodily functions; such spirituality provided man with a divine source of power in addition to vital and physico-chemical ones.

The first point to make about Hufeland's vital power is that he believed in power as a real entity in Nature; forces were therefore no merely heuristic concepts in natural philosopjy.[17] Indeed, in his analysis of what constitutes an individual organism, he had to deny the organism as a permanent, definable entity, for it seemed to him that the Leibnizean categories of change and power are the only truly ascertainable qualities in life; the organism continuously changes, just at Time does, and it is therefore forever elusive. In this argument, Hufeland's admission of the reality of power, arising from his denial of the existence of the individual as a permanent physical organism, seems to be analogous to Leibniz's admission of the reality of power, which was connected with his denial of the reality of Absolute Time. Leibniz had written:

> "*I find nothing so capable of being apprehended as force*. I believe that we must even have recourse to it in order to uphold the true (eucharistic) presence, which I admit I am not able to reconcile sufficiently well with the view that puts the essence of bodies in a completely empty extension".[18]

In an essay on dynamics, Leibniz had written that

> "It is yet appropriate to point out that force can be evaluated without taking time into consideration. For a given force can give a certain limited effect which it will never exceed, however much time be allowed. Whether a spring is let down suddenly or gradually, it will not raise a greater weight to the same height, nor the same weight to a greater height".[19]

In contrast with this reality and definability of force,

> "... motion is something successive, which consequently never exists, any more than time, because all its parts never exist together".[20]

In similar vein, Hufeland championed the reality of power in life:

> "The life of man, considered in a physical view, is nothing else than an incessant ceasing and being; a continual change of destruction and restoration; an everlasting contest of chemical, decomposing powers, with all the combining and creative vital powers ....
>
> What, then, in a common sense we call the life of a creature, considered as a representation, is nothing else than a mere

phenomenon, which has nothing peculiar or self-subsistent, but the active spiritual power which forms the grounds of it, and which binds and regulates the whole. All the rest is only appearance; a grand spectacle continued, where the thing represented does not remain the same a moment, but is incessantly changing".[21]

Life itself was therefore an incessant exertion of powers, and each vital action entailed a consumption of power as well as of living matter.[22] Such confidence in power contrasted with the reluctance of some of Hufeland's contemporaries to admit its reality; one such contemporary was Reil, who believed that, in the last analysis, all power could be reduced to chemical mixtures and particular forms of matter. Reil used the term *Lebenskraft* without assigning it any reality of its own. Thus:

"Force is a subjective concept, the form in which we think of the relations between cause and effect. If it were possible for us to think clearly of each body as it is, of the nature of all its constituents and their combination, and of their mixture and form, then we would not need the concept of force, which gives rise to quite a number of erroneous conclusions".[23]

Reil's use of the term *Lebenskraft*, despite his doubting its reality, is a fine testimony to the balanced, cautious approach he had to the problem, and it is therefore not surprising that historians have focussed on him. Hufeland's analysis, though more metaphysical than Reil's, was nonetheless a thoroughly reasonable one and contained some penetrating ideas that indicated how the topic could be investigated further. One of these ideas was the close affinity between vital and inorganic powers; in fact, Hufeland asserted that vital power had to be nourished continually for the organism to survive, and that nourishment was effected solely by light, heat and oxygene[24] [*sic*]. Indeed, Hufeland was one of the first physiologists to consider nutrition more from a dynamical point of view than from the traditional material one:-

> "I think I may with justice, therefore, assert that light, heat and oxygene are the real, proper nourishment and sustenance of the vital power. Grosser kinds of nourishment ... seem to serve rather for supporting the organs and repairing consumption".[25]

Similarly, bodily activity entailed primarily a consumption of power, whilst the physical wearing away of the organs was of only secondary importance.[26] Moreover, he interpreted an organism's physical size in essentially dynamic terms, so that the size of an oak-tree or an animal indicated primarily the quantity of vital power it contained and thus its life-span. This was expressed, for plants, by three pithy axioms:

I. Bulk shows greater provision of the vital or plastic power.

II. Bulk gives more vital capacity, more surface, more external acess.

III. The greater mass a body has, the more time is required before it can be wasted by its external and internal consumptive and destructive powers.[27]

The preservation of the bulk of a living organism necessitated a continuous replenishment of its power, for one idea which ran implicitly throughout his *Makrobiotik* was the inconceivability of creation of vital power *ab nihilo* or of the living system being a *perpetuum mobile*. Although he did not liken the organism explicitly to an inorganic mechanism, he liked to use the flame analogy, where the primary point was that the dynamic input had to equal the dynamic output if the flame was to burn steadily; the flame would die when there was insufficient input or excessive output of *power*; the consumption of the carbon of the candle, or of the *materials* of the living organism, was of only secondary importance.[28]

Another important feature of his physiology was that throughout life an organism had to compromise between the intensity of its actions, (that is, the rate at which it lived), and the duration of those actions (i.e. how long they could continue before the organism was totally exhausted).[29] All organisms had to rest regularly, for only thus could the consumption of vital power be retarded. All vital exertions drew power from an organism's stock of vitality, which had been imparted to it by its parents at its moment of conception, and its life-span was predetermined largely by the size of that stock. This particular argument did not take due account of his earlier assertion that vital power is continually replenished by light, heat and oxygene; but its usefulness lay in the ease with which physiologists could explain the different, and apparently predetermined, life-spans of different types of creatures; elephants lived longer than mice, rats, dogs etc., simply because elephants contained much more hereditary vital power than the smaller mammals, and could pass on more vital power to their offspring.

The differences of life-span among individuals of the same species could be explained dynamically with equal ease. One of the most important factors in this - indeed, the most important single factor in the case of man - was sexual indulgence. Hufeland's assertion of the intensely debilitating effect of this function was most impressive, both with respect to the breadth of empirical data that he used and the remorseless logic of his argument. Although Hufeland's discussion could not be called modern, it obviously retained its credibility for much of the 19th century because, by and large, it was the same argument as was used by the mid-19th century British and American physiologists discussed in Chapters 8 and 9. Even today, one can appreciate his logic:

> "What can communicate life, must also contain life. In the generative juices, the vital power is so concentrated that the smallest particle of them is able to call to life the future being. Can we imagine a greater balsamic for restoring and supporting our own vital power?
>
> We are sufficiently taught by experience that the body does not acquire its full solidity and consistence, until those organs /of generation/ have attained to perfection, and are in a condition to create this new kind of juices, and by this means to give expansion to new powers: the most evident proof that they are destined not merely for others but, in a particular manner, for ourselves and have so extraordinary an influence on our whole system, that they impress everything, as it were, with a new character never before felt ...."[30]

From such a passage we can see how useful the concept of power could be for physiology. There were certain vital processes which appeared so wonderfully inscrutable, so far beyond the reach of mere atoms, that the only agents likely to cause them were forces or powers. Only a dynamical agent could be sophisticated and potent enough to effect the well-nigh miraculous generation of an adult organism from a tiny spermatic animalcule and ovum. As various studies in the history of ideas on generation - by Needham,[31] Cole[32] and Bodemer[33] for instance - have shown, all attempts at accounting for generation by means of particles were running into serious difficulties in the 18th century, and those difficulties were not resolved even by the bio-chemical discoveries of the 19th century. One of the best ways to avoid those difficulties was to construct a dynamical theory of generation, for the only admissible agents in natural philosophy which could produce grand effects from tiny beginnings were powers and forces. Of course, any dynamical theory of generation had problems of its own, not the least of which was the difficulty of defining what was meant by 'force', (remember Reil's argument). But for men like Hufeland, who believed that forces were real and that recent researches in natural philosophy were holding out the promise of discovering their essential nature in the not-too-distant future, a dynamical account of generation was most convincing. Indeed, the dynamical theory of generation was to be tested experimentally in the 19th century by means of voltaic electricity; for instance, William Carpenter in his textbooks on physiology discussed the experiments of a Mr. Andrew Crosse (1784-1855) who in the 1830s had apparently generated tiny insects (*Acarus Crossii*) by passing electricity through salt solutions; Crosse's experiments were impressive and raised a storm of controversy in British scientific circles.[34]

In the *Makrobiotik*, Hufeland's view of the living organism seems to have been that it was an engine, in which the primary consideration was the economy of power; its only difference from an inorganic engine was its possession of an extra form of power - vital force - which, although a real, distinct form in its own right, nonetheless fitted into the universal laws of dynamics and did not usurp, but only modified, the physico-chemical powers. It was here that his metaphysics blended especially well with his science and made *Makrobiotik* so convincing; with great eloquence he asserted that vital power was of fundamental concern in physics and physiology, and that its origin was divine:

> "The vital power is, without dispute, one of the most general, the most incomprehensible, and the most powerful, of all the powers of Nature. It fills and gives motion to everything; and in all probability is the grand source from which all the other powers of the physical, or at least the organized, world proceed. It is that which produces, supports and renews everything; by it, the creation, after so many thousands of years, revives every spring with the same frehsness and beauty as when it first came from the hand of its Maker. It is inexhaustible and infinite - a real, eternal emanation from the Deity".[35]

Even though it was the most subtle, most pervading agent in Nature, Hufeland pointed out time and again that it seemed to have the closest affinity to light, electricity, magnetism[36] and heat,[37] and that the future of physiology and natural philosophy lay in the investigation of that affinity.

Hufeland's *Enchiridion Medicum* or *Manual of the practice of medicine: The result of fifty years experience*, was less concerned with physiological theory and more with the job of the physician. Hippocrates was his mentor; some passages were pure Hippocrates, both in ideas and in style, for instance:

> "Art is eternal, system transient.
> Art pertains to the internal sanctuary of
> man; system to time, whose product it is".[38]

Hippocrates was also his high-priest, for he had been the first to appreciate the vocationary nature and moral worth of medicine. Thus Hufeland advised:

> "Bear always in mind who you are, and what
> your office is. You are employed by God
> as a priest of the holy flame of life, and
> as administrator and distributor of the
> highest gifts, health and life, and of the
> secret powers which he has bestowed through-
> out Nature for the benefit of mankind".[39]

Occasionally, one gets the impression that it was not only Hippocrates, but also a twinge of *Naturphilosophie*, which guided him; for instance, despite the practical aim of the *Enchiridion* there were lengthy theoretical sections which expounded a wholly dynamical physiology, sometimes with considerable eloquence. In one notable passage discussing various methods of diagnosis, having enumerated the methods of chemical examination (which was still confined largely to urine analysis), examination of the animal electricity of the patient, (having Mesmer in mind), and examination by the stethoscope, he dismissed these three as only second-rate methods, since they disclosed

> "... more of a physical and material, than of a dynamic state, of the system, and are consequently of more value in regard to the natural history of the patient at the time, than of practical utility".[40]

Consequently, in this later treatise he again envisaged the organism as a system for the economy of power; and although the *Enchiridion* did not discuss as explicitly as the *Makrobiotik* the dependence of vital power on light, heat and oxygene, the idea of the correlation between vital power and the inorganic powers, upon which it was nourished, still seemed to be implicit.

One of the principal topics in the *Enchiridion* was fever; this was treated as a purely dynamical abnormality of the organism. Moreover, just as the various physiological manifestations of power were supposed to be forms of a fundamental power, so were different fevers only different forms of a basic fever. And just as power was one throughout Nature, so was fever one throughout all conditions of acute disease. Thus: "There is only one acute disease - it is fever".[41] And:

> "The practitioner has to look upon the original fever as a unity, as a phlogosis; and all the so-called species and sorts as mere deviations and modifications of this fundamental state, which they always retain as their base, and to which they may easily return again".[42]

Hufeland's solely dynamical view of health and disease turned his attention to the environmental and psychological needs of his patients in addition to their immediate medical conditions; we have seen how the same happened with John Brown and others in Britain, and Benjamin Rush in America. For Hufeland, this all-encompassing consideration of the patient had the valuable authority of Hippocrates, and it is hardly surprising that he advocated daily measurements of the barometer and thermometer, wind direction and intensity, atmospheric electricity and the likely effects of psychological factors such as anxiety and the threat of war.[43] The physician also had to understand natural philosophy and chemistry, at least in so far as they concerned life.[44] Hufeland believed these subjects to be intimately concerned with life for, in keeping with his *Naturphilosophische* background, which he rarely championed explicitly but which feels as if it was just beneath the surface of his writing, the vital power was envisaged as feeding upon the powers of Nature at large; and in exerting itself against Nature's powers, life seemed to consume itself. As was discussed in Chapter 10 of this thesis, power or force was a central concern of *Naturphilosophie*, and there were several natural philosophers who managed to produce valuable work under its aegis by holding its speculativeness in check. Everett Mendelsohn has cited Lorenz Oken, Richard Owen and Johannes Müller to that effect,[45] and it seems to me that Hufeland might qualify too, since his reputation and influence persisted long after his death. In fact, the *Makrobiotik* passed through an astonishing number of editions; it was last printed in 1975,[46] not as a medical curiosity but as a thoroughly valid guide to living. To a most impressive degree, Hufeland managed to temper his metaphysics with his experience as a working physician, and his dynamical ideas were therefore an integral - and are still a fairly unexceptionable - part of his careful, mainly empirically-based study of the prolongation of life. Hufeland's *Enchiridion* continued to be read and cited throughout Europe and North America throughout the 19th century for, as one physician wrote in 1931,

> "the closing chapters on medical ethics and the relation of the physician to the sick, to the public, and to his colleagues are most striking and constitute a classic in medical literature. The principles of the conduct of physicians, as laid down by Hufeland, became the code for the guidance of the profession".[47]

This being so, it was inevitable that Hufeland's dynamical ideas in physiology, even when outdated, had a wide audience; and it can be no coincidence that the writings on sexuality, for instance, by Orson Squire Fowler in America during the mid-late 19th century bore such resemblance to Hufeland's ideas on the same topic. Fowler was exceptional in not citing Hufeland, for most of the physiologists writing in English after c.1830 and who have been examined in this thesis, cited him approvingly.

NOTES TO CHAPTER 12.

1. There is, however, a paper on Hufeland, which focusses on his medical ethics: I.A. Abt, 'Christopher Wilhelm Hufeland', *Ann.med.hist.*, 1931, N.S.3:27-38. For my introduction to Hufeland I am indebted to Miss Marianne Winder at the Wellcome Institute for the History of Medicine, for she made me aware of Hufeland's likely significance for my thesis even before I came across references to him in other 19th century physiological writings.

2. Most of my biographical data has been obtained from Klaus Pfeifer's excellent book, *Christoph Wilhelm Hufeland, Mensch und Werk*, Halle, VEB Max Niemeyer Verlag, 1968.

3. E. Baymann, *Christoph Wilhelm Hufeland und die praktische Medizin*, Düsseldorf, *gedrückt mit Genehmigung der Medizinischen Akademie in Düsseldorf*, 1964, p.4.

4. *Op.cit.*Note 1 above, p.38.

5. F.H. Garrison, *An introduction to the history of medicine*, 4th edition, revised, Philadelphia & London, W.B. Saunders Co., 1929, p.366.

6. C.W. Hufeland, *Makrobiotik: Die Kunst, das menschliche Leben zu verlängern*, Jena, '*In der akademischen Buchhandlung*', 1796. I have used the earliest English translation for the extracts in this chapter: C.W. Hufeland, *The art of prolonging life*, London, J.Bell, 1797.

7. C.W. Hufeland, '*Magnetismus. Medicina magica. Bewirkt der Magnetismus eine Erhölung oder Erniedrigung der menschlichen Natur? oder Betrachtung des Sonnambulismus von seiner moralischen und reliogiösen Seite*', *Journal der practischen Arzneykunde und Wundarzneykunst*, 1822, 54:Part VI:3-21. Discussed in Pfeifer, *op.cit.*, Note 2 above, and in Baymann, *op.cit.*, Note 3 above.

8. C.W. Hufeland, '*Die Homöopathie*', *Journal der practischen Arzneykunde und Wundarzneykunst*, 1826, 62 (I):3-28; also 1828, 66 (II):61-65.

9. C.W. Hufeland, *Enchiridion Medicum, oder Anleitung zur medizinischen Praxis*, Berlin, Jonas, 1836. I have used the translation by C. Bruchhausen and R. Nelson, *Manual of the practice of medicine. The result of fifty years experience*, London, H. Baillière, 1844.

10. See R. Heller, 'Johann Christian Reil's training scheme for medical auxiliaries', *Med.hist.*, 1975, 19:321-332, esp. pp.323-324 for discussion on Hufeland.

11. M. Teich, 'The historical foundations of modern bio-chemistry', in J. Needham (editor), *The chemistry of life*, Cambridge, C.U.P., 1970, pp.171-191.

12. *Ibid.*, p.172.

13. M. Florkin (editor), *Comprehensive biochemistry*, Vol.30, *A history of biochemistry, Part 1*, Amsterdam, Elsevier Publishing Co., 1972, p.219.

14. N.G. Coley, *From animal chemistry to biochemistry*, Amersham, Bucks., Hulton Educational Publications Ltd., 1973, pp.81-82.

15. *Ibid.*, p.81.

16. Hufeland, *op.cit.*, Note 6 above, pp.ix-x.

17. *Ibid.*, pp.1 & 2.

18. Gottfried Wilhelm Leibniz, in a letter to Jacques Benigne Bossuet, Bishop of Meaux, in 1694. Quoted in Pierre Costabel's excellent book *Leibniz and dymanics, the texts of 1692*, trans. by R.E.W. Maddison, London, Methuen, 1973, p.65.

19. *Ibid.*, p.63.

20. *Ibid.*, p.64.

21. *Op.cit.*, Note 6 above, pp.183-184.

22. *Ibid.*, p.190.

23. J.C. Reil, '*Von der Lebenskraft*', *Arch.Physiol.*, 1796, 1: 46. Cited in Teich, *op.cit.*, Note 11 above, p.173.

24. *Op.cit.*, Note 6 above, p.52.

25. *Ibid.*, p.56.

26. *Ibid.*, p.60.

27. *Ibid.*, p.82.

28. *Ibid.*, pp.185-186.

29. *Ibid.*, pp.66-72.

30. *Ibid.*, pp.228-229.

31. J. Needham, *A history of embryology*, 2nd edition, Cambridge, C.U.P., 1959.

32. F.J. Cole, *Early theories of sexual generation*, Oxford, Clarendon Press, 1930.

33. C.W. Bodemer, 'Regeneration and the decline of preformationism in eighteenth century embryology', *Bull.Hist.Med.*, 1964, 38:20-31, and 'Concepts of reproduction and its regulation in the history of western civilization', *Contraception*, 1976, 13:427-446.

34. Andrew Crosse had studied chemistry and natural philosophy, among other things, at Oxford, and on settling down to the life of a country gentleman he began to do electrical experiments. In 1837, whilst doing experiments on electro-crystallization, he noticed the appearance of minute insects in the solutions through which voltaic electricity was passing. The insects were identified as belonging to the genus Acarus and were named *Acarus crossii*. On publishing his discovery he was, in his own words, "met with so much virulence and abuse, ... that it seems as if it were a crime to have made them".

35. *Op.cit.*, Note 6 above, pp.40 & 41.

36. *Ibid.*, p.43.

37. *Ibid.*, p.52.

38. *Op.cit.*, Note 9 above, p.75.

39. *Ibid.*, p.77.

40. *Ibid.*, p.69.

41. *Ibid.*, p.79.

42. *Ibid.*, p.83.

43. *Ibid.*, p.31.

44. *Ibid.*, p.32.

45. Everett Mendelsohn, 'Explanation in nineteenth century biology', in R.S. Cohen and M.W. Wartofsky, *Boston studies in the philosophy of science*, vol.2, New York, Humanities Press, 1965, pp.127-150. This quotation p.132.

46. C.W. Hufeland, *Die Kunst, das menschliche Leben zu verlängern, Makrobiotik*, with an introduction by K.E. Rothschuh, Stuttgart, Hippocrates Verlag, 1975.

47. *Op.cit.*, Note 1 above, p.31.

CHAPTER 13. On Power in the physiology of Johannes Peter Müller, (1801-1858)

"When one comes into contact with a man of the first rank, one's spiritual scale is changed for life. Such a contact is the most interesting event that life can offer." /Hermann von Helmholtz, probably thinking of Johannes Müller/

In 1819, Johannes Müller began his medical studies at the University of Bonn. The teachers there had been selected by the Prussian Ministry of Education largely for their adherence to Schelling. It was therefore inevitable that Müller would be moulded into a *Naturphilosoph*. *Naturphilosophische Medizin*, which had John Brown's excitation theory as one its main components, was well represented at Bonn; there were the physiologists Christian Friedrich Nasse (1778-1851) and Philipp von Walther (1782-1849), Karl Wilhelm Gottlob Kastner (1783-1857) a friend of Schelling, and the botanist Nees von Esenbeck (1787-1837) a friend of Goethe.[1]

A turning point came when, having graduated in 1822, Müller moved to Berlin to study under Karl Rudolphi (1771-1832), then Germany's most eminent anatomist, who had been trying to free natural philosophy from the "turbid mire of mysticism" and to establish a rigorous methodology.[2] By the late 1820s Müller had learnt enough from Rudolphi to be acquiring a reputation in his own right in experimental physiology and anatomy, and in 1826 he was appointed to the chair of medicine in Bonn. However, his years under Rudolphi when he had been acquiring his teacher's methods, cannot have gone too smoothly;

for instance, his first piece of important research was on psychology, and he recalled later that his teacher had frowned upon his preoccupation with that

> "more abstract subject of the physiology of the mind, rather than with the investigation of the anatomy of the organs of the senses, such as that of the eyes of insects and spiders".[3]

Actually, Müller soon adopted a more orthodox approach to the study of how man acquires his knowledge about the external world, as evidenced by his first publication, *Zur vergliechenden Physiologie des Gesichtssinnes des Menschen und der Thiere* (1826),[4] (On the comparative physiology of sight in man and in animals). It marked not only the start of his career but also the opening of an era in the physiology of sense-perceptions, for its thesis was:

> "We want to say, right at the beginning, the basic idea of all physiological investigation, not only of vision but of all the other senses, which we cannot repeat often enough ... is that the energies of light, dark and colour are not immanent in external things ... but in the visual substance itself".[5]

Essentially, he argued on the basis of his own experiments that the qualities we attribute to the external world are largely, or even wholly, created by our own organs of sense; that, as Kant had proposed, there is a gulf between phenomena and noumena that in many, or even all, cases cannot be bridged by the human understanding.

In the same year he published another work, seldom even mentioned by historians of physiology, entitled *Ueber die phantastischen Gesichtserscheinungen*,[6] (On fantastical sight perceptions) which had relied on a series of difficult self-experimentation and was a much more subjective and metaphysical work than the other. In the foreword he explained the connection between the two, (and since this work has never been translated and is so seldom discussed, I might be excused a lengthy extract):

> "The present investigation is to be regarded as a continuation of the earlier physiological work of the author on the faculty of sight (*On the comparative physiology of the sight-faculty of man and of animals*, Leipzig, 1826). It deals with the sight-faculty with respect to its higher social bearing upon the organs, whose life-form we call psychic, spriritual. For the author, the soul is only one special form of life amongst many, which is amenable to physiological research; he retains the conviction, therefore, that physiological research must itself, in the last analysis, be psychological. The doctrine of the life of the soul as a particular life-form of the organism is therefore only a part of physiology, in the widest meaning of the word.... Should the author explain himself briefly on this issue, which seems to him a scientific (*wissenschaftliche*) physiological treatment of psychology, he would declare that, though guarding himself well against the suspicion of Spinozism, he has no doubts upon the last three books of the *Ethics* of Spinoza, which dealt with the violent emotions and whose psychological content can be seen to be separate from his other teachings.... [they] provide at least an effective account of the method and purport of life, which one cannot say for most psychological treatises".[7]

In other words, the young Müller considered Spinoza's psychology to be of heuristic value, even though he would not admit it as necessarily true, a typically Kantian position. In the next few sentences, he revealed how much he was also indebted to *Naturphilosophie*:

> "Here, the author has nothing to say about the life-forms that we call spiritual [or immaterial]; but the life-form of sensitivity, which was his specific object of study, stands among all physiological functions in such a direct, transforming relation to spiritual life, that the physiological study, if it succeeds, cannot be without psychological implications. Already in earlier physiological work, the author believes he has indicated many psychological implications. This relationship is developed even more significantly in the present paper, whose task is precisely to investigate the sight-faculty in its transformations with spiritual life. May this work contribute a little to leading psychological research back from the sterile ground of so-called empirical psychology, and on the other hand from the all-too-easy and peremptory speculations on life, towards fruitful results".[8]

As we have seen, the topic of transformation among Nature's dynamical agents was central to *Naturphilosophie*. Here was Müller championing that idea more forcefully than he would ever again and also revealing that it lay at the very root of his newly found principle of specific nerve energies. This is NOT to say that the only basis for that principle was metaphysical; his autoexperimentation had given him another basis, even though such experiments were difficult to perform properly and even more difficult to interpret. (Remember that at about this time Roget was doing similar experiments on himself and had found them difficult to interpret conclusively. A little earlier, the young Czech physiologist, Purkyně, had done similar autoexperiments and made himself violently ill thereby.[9]) The question of the origin of Müller's specific-energies principle has been asked by physiologists, philosophers and historians from that day to this. According to J.T. Merz in his *A history of european thought in the nineteenth century*, the answer given by Wilhelm Wundt (1832-1920) the physiologist,[10] and Helmholtz was Kant's epistemology;[11] they regarded it as an attempt to verify the *a priori* character of human understanding by a piece of carefully designed physiological research. Their interpretation seems to me to be correct, even though we must bear in mind that Wundt rejected Müller's principle, proposing instead a law of neural functional indifference,[12] and their views were suspiciously typical of the 1870s and 1880s, a period of enthusiastic neo-Kantianism[13] quite different from Müller's time.

Let us briefly examine the two components of Müller's principle, namely specificity and his word *energie*. It has been suggested that he obtained the actual word *Energie* from either Purkyně, (who admired and might have obtained the word from Goethe; Müller had read Purkyně's writings on the subjective phenomena of vision[14]), or from Magendie, who used the word in describing the sensibility of the nasal cavity.[15] Moreover, as we have seen, the concept of excitation and the word 'energy' were part of the Brunonian contribution to German physiology in the early 19th century. Müller was thus using a current term when he spoke of *Energie*, a term that had not yet been given a precise meaning and could still be synonymous with more traditional terms such as Haller's 'irritability' and 'sensitivity'.

However, whereas irritability and sensitivity were general properties, Müller definitely meant his *Energieen* to be specific. One scholar has suggested that we must turn to Goethe's theory of colour for the source of this specificity.[16] Goethe had asserted that light is essentially white and that the so-called seven primary colours that Newton had obtained with his prisms were artefacts.[17] Goethe's method had been partly experimental, but largely psychological; like Purkyně, Müller and Roget, he believed that certain aspects of sense-perception were not amenable to mere physical experimentation, such as Newton had used. Müller thought highly of Goethe's work on colour:

> "I have no second thoughts about admitting how very much I owe to the inducements of Goethe's colour theory, and can fairly say that without many years of studying it, together with personal observation of the phenomena, the present researches would not have arisen".[18]

Goethe's influence is also discernible in Müller's inaugural lecture in 1824, when he declared that

> "plain observation in anatomical investigation is far more splendid and superior to the reckless and often deceitful physiological experiment";[19]

he also declared that a certain type of investigation, namely the sectioning of nerves, could lead to '*Urphaenomena*'[20] in physiology. In 1824 François Magendie had published his experiments on nerve-sectioning.[21] Müller read them but could not believe his claim that pricking nerves after they had been sectioned *in vivo* did not elicit their customary responses.

The reason for his scepticism was his own, and Goethe's, belief based on their own autoexperiments that the sensation of light could be produced by both internal and external causes. By this belief, which he described as "the metamorphosis of the visual substance",[22] in imitation of Goethe's *Versuch die Metamorphose der Pflanzen zu erklären*,[23] he meant that sensations are specific qualities of nerves themselves.

I have discussed these two 1826 treatises in some detail, not only because they contained Müller's seminal discovery of specific sensitivities of nerves, which (as I shall argue later) led on to his developing a strongly dynamical general physiology and a special interest in the dynamical relationship between the external world and the whole living organism, but also because they illustrate two key characteristics of his life's work: the use of carefully planned experimentation, as found especially in the *Vergleichende Physiologie*, and his deep philosophical roots, as evidenced by the *Phantastischen Gesichtserscheinungen*. The roots never atrophied although they did change direction, for by 1827 he apparently abandoned *Naturphilosophie* though retaining his deep interest in philosophy itself. Evidence of the latter was his frequent citation of Spinoza and Kant in his *Handbuch der Physiologie des Menschen* (Treatise on human physiology), (vol.1, 1835, vol.2, 1837). Actually, his philosophical mentors went even earlier: most historians who bother to mention the *Phantastischen Gesichtserscheinungen* cite it as I have. But there was a second part to its title: *Eine physiologische Untersuchung, mit einer physiologische Urkunde des Aristotles über den Traum*. Müller considered Aristotle's treatise on dreams to be a truly physiological one and essentially correct.[24] We shall see Aristotle re-emerging in the *Handbuch* with amazing vitality.

In 1833 Müller succeeded Rudolphi ~~to~~ in the Berlin chair of anatomy. The post had been offered to Tiedemann, and when he declined Müller proposed himself; among the skills he declared to be necessary, and which he possessed, were a comparative approach to anatomy, experience in physiological research, microscopical techniques and embryology, and the ability to teach. Indeed, his evaluation of himself was accurate, for during the 1840s and 1850s he was virtually the high priest of the biological sciences in Germany, not so much for his own discoveries, though they were impressive, as for the training he provided for a whole generation of men, who went on to do their own physiological research and to occupy the professorships in physiology at many of the German, Austrian and Swiss univerties.[25] In this respect, Müller occupied a pivotal position in 19th century physiology, just as Liebig occupied a pivotal position in chemistry. Like Liebig and almost contemporaneously, Müller also developed an outlet for the publication of research results: he became the editor of the *Archiv für Anatomie, Physiologie und wissenschaftliche Medizin*, which had been established in 1796 as the first journal devoted solely to physiology - it was then called *Archiv für die Physiologie* and was ailing badly when Müller took it over and restyled it. That and his *Handbuch der Physiologie* were filled with contributions made by him, his colleagues and students. The late 19th century physiologist, Max Verworn (1863-1923) looked back on him as "one of those monumental figures that the history of every science brings forth but once".[26] Indeed, historians of physiology have always acknowledged his stature, but sometimes for different reasons; for instance, Hans Driesch in his *Der Vitalismus als Geschichte und als Lehre* (1905)[27] and *Science and philosophy of the organism* (1929),[28] and L. Richmond Wheeler in his book *Vitalism* (1939),[29] praised him largely for his scientific contribution to the doctrine of vital force. Wheeler saw the principle of specific nerve energies as evidence for vitalism, for it implied that things in themselves, the noumena, could never be truly experienced and all knowledge therefore has a subjective and vitalistic basis. Most recent historians would disagree with Wheeler's use of Müller, and it seems to me that the principal import of Müller's work on sense-perception, as he saw it, was:

1) That the nature of causality, particularly between phenomena outside of ourselves and our perceptions of them, was much more complex than had been commonly thought hitherto; that, as Kant had said, a specified cause need not have a specific effect, but that different effects could arise according to the conditions within which the cause acts.

2) That the problem of human knowledge is immensely complex and perhaps inscrutable, and therefore that mere experimentation and data-collecting will never suffice in science; the scientist must employ the tools of philosophy, psychology, intuition and scientific disciplines other that his own.

As we have seen, both these themes had been aspects of Kant's analysis of the acquisition of knowledge.

Parts of Driesch's and Wheeler's appreciations, however, did not depend on their own wish to vindicate vitalism and are in agreement with more recent historians; for instance, Owsei Temkin,[30] Gottfried Koller,[31] Walther Riese[32] and Everett Mendelsohn.[33] Thus, Wheeler realized that Müller was not a traditional vitalist, for he rejected abiogenesis and the doctrine of a specific living matter and tried, as much as was reasonably possible, to explicate organic activities in physico-chemical terms.

Because of his eagerness to use whatever techniques were at hand, and for other reasons mentioned above, it is easy to see why Müller's *Handbuch* became a best-seller, set the pattern for a new genre of physiological treatises, was translated extensively and was still hailed as a masterpiece by researchers into sense-perception in the 20th century, for instance by Ernst Mach.[34] However, the writing was already on the wall before volume two came out: as Mendelsohn mentions,[35] during the early 1830s while he was writing volume one, Müller was still a staunch vitalist; by the time that volume two appeared, he was no longer so sure of his vitalism, but he could never shake it off. One example of the less physico-chemical approach in volume one was his discussion of specific nerve energies; the principle was now formulated in more neuro-anatomical terminology than it had been in the *Phantastischen Gesichtserscheinungen*, but he was still thinking in terms of vague energies and excitabilities rather than of material changes in the body's tissues as he was to later. These *Energieen* were not defined rigorously, since he did not regard them as merely physical agents. Perhaps that was why his pupil, Helmholtz, rarely used the word

Energie when discussing qualities of visual sensation, but preferred to call them signs, (*Zeichen*). Eventually, *Energie* came to mean something purely physical for Helmholtz, as when he used it in his 1847 paper '*Ueber die Erhaltung der Kraft*'. Yet, although departing from his teacher's broad and imprecise idea of the word, Müller's *Energie* was clearly his starting point and was important in the development of his ideas which led ultimately to the conservation of force/energy. Moreover, it seems to me that in volume two of the *Handbuch*, Müller's use (even if not his actual concept) of *Energie* became more refined and therefore perhaps more acceptable to Helmholtz and his fellow students.

One historian of physiology has written that, although Müller was clearly a vitalist, he led his students into fields where they developed a radically new idea of what constitutes a proper physiological explanation.[36] Students like Theodor Schwann, Emil du Bois-Reymond and Hermann Helmholtz emerged from their initial research studies believing that vital functions could be explained solely by physico-chemical forces and laws. In the next few pages we shall examine a few instances of this.

In Volume 1, Book 3, of the *Handbuch* Müller discussed the physiology of the nerves. There he set a challenge for physiologists, a challenge so explicit that its impact on his students and readers can scarcely be doubted; indeed, we know how soon it was to be taken up by his pupils. The issue was animal excitability:

> "Physiologists have not, however, merely to ascertain the laws governing this general property, which unfortunately was the sole

object whcih occupied the attention of /John/ Brown and his followers; but to investigate the peculiar forces themselves which are susceptible of this excitation, and in this there is a great field opened for experimental science. In inquiring into the nature of the forces resident in the nerves, it is necessary to study the action of all kinds of stimuli upon them - a method of enquiry which acquires for physiology an experimental certainty similar to that which the sciences of physics and chemistry in reference to inorganic bodies enjoy. In chemical processes, reagents give rise only to products, combinations and decompositions; applied to organic bodies and especially to the nerves, their effects, however various they may be, are never other than manifestations of the proper forces of the bodies acted on, or modifications of their forces. It will be seen that all influences acting on the nerves either excite them or produce an altered state of their excitability; ... the most different causes produce the same effect, because that on which they act possess but one kind of excitable force, and because agents in themselves the most different act here by virtue of the same quality, that of stimuli".[37]

Already this issue had been studied with respect to the physiological effects of galvanism, and Müller praised especially the writings of Galvani, Alexander von Humboldt, Volta, Johann Ritter (1776-1810), Jan Purkyně the young Czech physiologist, and Richard Fowler;[38] Fowler's researches would have been available to him in German[39] as well as English.

Müller's own explanation of these galvanic studies was wholly physical and his *Energie* seemed not to have any metaphysical implication worth mentioning; the central idea was that the activities of nerves always entail an expenditure of power which has to be replenished continually from external sources, especially by nutrition; nutrition was therefore a dynamical process as well as a material one:

> "All stimuli, which by producing changes in the matter of nerves excite reaction of them, are also capable of modifying their state of excitability. Reaction is always attended with an expenditure of power; it is the result of the material change; and the longer the excitement is continued, the greater is the change produced.... The daily changes in the system, consequent on the action of stimuli, are counterbalanced by the processes of nutrition".[40]

In Volume 2, which appeared two years after Volume 1, he extended this dynamical explanation to all vital activities, again emphasizing that two types of change occur: i) a change in the chemical composition of the active tissue itself; and ii) A diminution in its power. As is well known, these became key ideas in Liebig's theory of vital activity, although Liebig did not acknowledge any debt to Müller; they also influenced his pupil, Helmholtz, for one of his earliest, important experiments was designed to test the hypothesis that vital activities always entail chemical changes in the tissues concerned; that was his 1845 study on the excitation of excised frog leg muscles.[41] Müller's own succinct account of his hypothesis was that

> "This constant reanimation of the tissues by the general vital stimuli ordinarily renders them capable of a proportionate exercise of their functions; but if their action is increased and accelerated, subsequent rest is necessary to restore as much power for new action as has been thus consumed. Generally, in the healthy state, just as much power is generated in a certain space of time as has been exhausted by the exercise of the functions; but there are cases in which the nutrition of the organ becomes gradually increased, while the state of action is either equal or regular, or alternating with rest. This is the case in youth ...."[42]

For Müller, the only truly vital stimuli were external heat, air, water and food; these alone were the causes of the manifestation of life and of the increase of vital force.[43]

From such passages one gets the impression that one of Müller's intentions was to argue against the living organism as a *perpetuum mobile* or as a creator of its own power; actually, this became explicit in his discussion of the nature of the organic force, where he also seemed sometimes to propose the existence of a fundamental type of vital force, a *vis essentialis*, to which all modes of vital activity might be referred.[44] Such a force was only a probability for Müller, yet one aspect of vital forces of which he was sure was that they are never *sui generis* but always require replenishment from external sources of power. Between inorganic and organic forces there was seen to be a fundamental interchangeability or correlation; to suppose the contrary to be true,namely that organic power could be created, struck him as absurd. This idea is so important that it merits extensive quotation:

> "... the source of the increase of the organic or vital force seems, therefore, also to lie in the organization of new matter; and this being admitted, it must be allowed that plants, while they form new organic matter ... are also endowed with the power of increasing the organic force from unknown external sources, while animals also in their turn would generate the organic force from their nutriment under the influence of the vital stimuli, and distribute it to the germs during propagation. Whether during life the organic force, as well as the organic matter, is constantly suffering destruction is quite unknown. This much, however, seems certain, that at the death of organic bodies, the vital force is resolved into its general natural causes, from which it appears to be generated anew in plants. If this increase of the vital principle in existing organized bodies from unknown sources in the external world be not admitted, it must be supposed that the apparently endless multiplication of the vital force in the process of growth and in propagation is merely an evolution of germs encased one within the other, or it must be admitted that the division of the organic force which takes place in propagation does not weaken its intensity - a supposition which appears absurd. But the fact would still remain, that by the death of organized bodies, organic force is constantly becoming inert, or resolved into its general physical causes".[45]

As exemplified in this passage, the way whereby Müller formulated his particular version of the Conservation of Energy was by considering the ancient problem of generation; the essential problem, as he saw it, was that in propagating their own kind, living organisms must share out their power; yet the intensity of their power never seems to diminish from one generation to the next; how can something divide itself in such a way that each fraction is as large (or intense) as the original whole, without positing a perpetual creation? Clearly, the only acceptable alternative to perpetual creation was continuous nutrition. That seems to me to be the significance of nutrition in his physiology.

Müller found this account satisfying not only from a purely physiological, 'scientific' point of view, but also philosophically. As we have seen, in 1826 one of his philosophical mentors was Benedict Spinoza, for so long as one distinguished Spinoza's philosophy from his theology, Müller believed him to be a most valuable mentor. Towards the end of Volume 2 of the *Handbuch*, Müller wrote a section on 'Cosmological systems', where he discussed the two principal hypotheses on the connection of life and mind with matter, at the point where that issue passed "beyond the domain of empirical physiology".[46] One hypothesis was Plato's theory of innate and transcendent ideas; this was the commonly accepted hypothesis on the relation of the mental and vital principles to the body, for upon this was based the Christian belief that the vital essence or spirit leaves the body at its death and returns to its divine source. For Müller, the main difficulty in this doctrine was that life and mind, not being latent properties of matter, could not be replenished from any physical source during the propagation of organic creatures,

> "... which, contrary to every attribute of matter, renders them capable of division, *ad infinitum*, without any diminution of their power or intensity. Such a property it is certainly difficult for the mind to conceive".[47]

The second hypothesis, the pantheistic view of a universal spirit, was easier to imagine and rendered the growth of organic force during the propagation of life perfectly intelligible. Although this was Spinoza's doctrine, Müller chose not to quote him but quoted Giordano Bruno as his sole authority, (presumably because Bruno was the earlier and because Spinoza was still theologically unacceptable and smacked of total atheism in certain circles). From Bruno's theory, Müller concluded that organisms, wherein vital and spiritual phenomena manifest themselves through definite structures and chemical compositions, have emanated from the creative spirit of God. As soon as inanimate matter comes within the influence of this creative spirit acting in the guise of vitality, its capacity for life, hitherto latent, becomes manifest. Consequently, the assimilation of new matter by an organism gives rise to an increase of its organic force, and with this is combined its capability of multiplication by division of itself. To all this Müller added that

> "Phenomena analogous to such a conditional manifestation of a principle of vital content in all matter, are known in physical science.

Forces or principles such as electricity and light, for example, which are present in a latent state in bodies, are manifested when these bodies are subjected to certain conditions".[48]

This view agreed neatly with a theory on vitality that the anatomist Reil had proposed, and that Müller discussed elsewhere.[49] Reil had suggested that the characteristics and powers of organic systems are the results of the mode of combination of the chemical elements, that form and composition are the primary factors in differentiating life from non-life, but that until such factors have been truly elucidated, physiologists may use the term *Lebenskraft* as a provisional heuristic device. Some men, Rudolphi for instance, regarded Reil's idea as a masterpiece, but not Müller. As Müller pointed out, the chemical composition of an organism must be the same immediately after death as immediately before; therefore, the life-principle would have to be something over and above mere form and composition; Reil would want it to be something material, but Müller leaned towards something dynamical. With characteristic frankness Müller admitted that

"Whether this principle is to be regarded as imponderable matter, or as a force or energy is just as uncertain as the same question is in reference to several important phenomena in physics; physiology in this case is not behind the other natural sciences, for the properties of this principle in the functions of the nerves are nearly as well known as those of light, caloric and electricity in physics".[50]

Clearly, he believed that much the same type of research was needed in physiology as in physics to elucidate the nature of the imponderable agents, and that these disciplines would be of mutual benefit to each other; perhaps, even, the enigma of the unknown principle that had had to be used to supplement Reil's view would be solved. As we know, this line of thought was soon developed by some of his pupils; by 1841, Du Bois-Reymond was quoting with approval the idea of the Marquis Dutrochet (1776-1847), that

> "The more one advances in the knowledge of physiology, the more reasons one will have for stopping to believe that the phenomena of life are essentially different from the physical phenomena".[51]

Inevitably in this thesis, Müller is being discussed partly because Helmholtz was his pupil, and we want to know what Helmholtz's intellectual debts to him might have been. It seems to me that Helmholtz took over not only his ideas in, and approach to, physiology, but also his more purely philosophical interests, and that their intellectual relationship therefore existed in several fields. This is not generally discussed by historians of 19th century German physiology, although inevitably Leo Koenigsberger's standard biography[52] of Helmholtz did explore this theme. In what follows I shall be drawing on Koenigsberger, although some of my contentions shall be my own.

We have already seen how well Müller thought of Bruno and Spinoza. In the section 'On the distinction between mind and life' in the *Handbuch*, Müller revealed the strongly favourable impression that Locke, Hume, Kant and Aristotle made on him. It can scarcely be a coincidence that Helmholtz discussed Spinoza in Volume 3 of his *Handbuch der physiologischen Optik*, or that his philosophical inclinations were similar to Müller's, for instance, his general agreement with Locke. Both were greatly indebted to Kant for their *Weltanschauung* and believed his philosophy a valuable guide in scientific work;[53] in a letter in 1857 Helmholtz wrote:

> "It seems to me a favourable moment for voices of the old school of Kant and the elder Fichte to obtain a hearing once more. The philosophical vaporing and consequent hysteria of the 'nature-systems' of Hegel and Schelling seem to have exploded, and people are beginning to interest themselves in philosophy again .... Philosophy finds its great significance among the sciences as the theory of the source and functions of knowledge, in the sense in which Kant, and so far as I have understood him, the elder Fichte, took it".[54]

Such faith in the scientific import of pure philosophy Helmholtz reiterated in a letter approximately twenty years later:

> "I believe that any German University that had courage to appoint a scientific man with an inclination for philosophy to its Chair of Philosophy would confer a lasting benefit on German science".[55]

In my opinion, one of the most interesting and difficult to calculate examples of Müller's belief in the importance of studying the ideas of his predecessors was his use of Aristotle. We have seen that he appended his own translation of Aristotle's treatise on dreams to his work *Ueber die* *phantastischen Gesichtserscheinungen* in 1826; he called Aristotle's treatise "*eine physiologische Urkunde*", and took it as his philosophical and physiological *vade mecum*. More than a decade later, in his *Handbuch*, he reaffirmed his immense admiration for it:

> "Aristotle's treatise on dreams contains views in themselves more correct, and stated in a more scientific form /than any more recent treatises/. His explanation of spectral appearances as a result of internal actions of the sense of vision, is quite on a level with the present state of science. He adduces, indeed, the observation made since by Spinoza, that images seen during sleep can still be perceived in the organs of vision after waking; and the varying colours of the ocular spectra produced by gazing at the sun were well known to him".[56]

He went on to say that despite the recent tendency to fragment science into separate disciplines, there remained the important, general task

> "... to test the theories of fundamental phenomena, more especially of those which interest different sciences, such as the actions of light,on organic beings. But this would be a task of extreme difficulty ...."[57]

It was no coincidence that his pupil, Helmholtz, would devote much of his physiological career to this very task, physiological optics. Müller warned that the study would be difficult and time-consuming, and it took Helmholtz more than a decade to write his three volume work on it. Neither was it a coincidence that in the preface to Volume 3, Helmholtz also discussed the fragmentation of science, to which he attributed his predecessor's lack of success in studying visual perception; he asserted that only by studying the subject from all aspects had he achieved anything worthwhile. The only omission from his preface, compared with Müller's passage quoted above, was that he did not cite Aristotle.

There is a most interesting affiliation (which, to my knowledge, no other historian has commented upon) between Müller's *Handbuch* and Aristotle. It occurs in the sections 'Of the senses'[58] and 'Of the mind'[59] in Volume 2 of the *Handbuch*. There, Aristotle's treatise on dreams *De somniis* and his *De anima* are cited glowingly. Moreover, not only are their ideas in general agreement, but the very lay-out of Müller's discussion parallels Aristotle's lay-out. Müller's 'Of the mind' begins with a survey of his predecessors in the field - Plato, Pythagoras, the new-platonists and the pantheists, Anaxagoras, Heraclitus and Bruno. Aristotle surveys his predecessors in greater detail.[60] Then they discuss the hypothesis that "like attracts like", Aristotle mentioning Plato's use of it in *Timaeus*[61] and Müller citing Hegel.[62] Already in the 'Prolegomena' to the *Handbuch* he had discussed this hypothesis, saying how handy it would be to make a single observation of an organic attraction between similar living germs.[63] The next topic they both discuss is the homogeneity and distribution of the soul within the living body; their arguments are again similar. In discussing the senses they follow the same order: Aristotle deals firstly with sense-perception generally, then sight, hearing, smell, taste and touch (all in Book 2 of *De anima*), and finally with mind and motion (Book 3). Müller parallels him, the general discussion and the five sences occupying 'Book 5. Of the senses', and mind and motion occupying 'Book 6. Of the mind'. Both begin the discussion of touch by wondering whether it is confined to particular, very localized parts of the body or is distributed over its entire surface. Aristotle leaves this question unanswered, for

> "... we are unable clearly to detect in the case of touch what the single subject is which underlies the contrasted qualities and corresponds to sound in the case of hearing. To the question whether the organ of touch lies inward or not (that is, whether we need look any farther than the flesh), no indication in favour of the second answer can be drawn from the fact that if the object comes into contact with the flesh, it is at once perceived".[64]

Müller's discussion starts off by seemingly answering Aristotle's question; he says that the sense of touch is possessed by all parts of the body which can register the sensation of touch or of pain or pleasure, heat or cold. Indeed, to read the *De anima* and Müller's discussion side by side, one cannot avoid the impression that Müller deliberately modelled his discussion on Aristotle's and had set out principally to bring Aristotle's treatise up-to-date.

In his dynamical view of the organism Müller distinguished two type of stimulus, and in this he was partly original. The less important type consisted of agents that induced or prodded an organism into activity; by and large, tnese were what the 18th and early 19th century physiologists had in mind when using the terms "stimulus", "stimulant" or "excitant"; and although they were considered to be necessary for the manifestations of life, they were only supposed to keep the organism awake (so to speak), and did not provide the organism with power to keep it going. Müller's second type of stimulus was more important to him; these were the truly vital stimuli which provided an organism with power, as well as prodding it into action; they produced two types of effect - the purely chemical, material changes that are seen to occur throughout life, and the interchanges of force that occur between a living system and its environment. Such vital stimuli were therefore the *pabulum vitae*, in both a material and dynamical sense; by their means, Müller believed one might reasonably compare the organism with a piece of pure mechanism or a flame:

> "The external conditions which are necessary to life - caloric, water, atmospheric air and nutriment - at the same time that they maintain life, induce constant changes in the composition of the organized body; themselves combining with the body, while certain old components are decomposed and cast off. These external agents have been called vital stimuli .... These vital stimuli produce the phenomena of life by effecting material changes, by producing an interchange of ponderable and imponderable matters ....
>
> The stimuli are, as it were, the external force which sets in motion the wheels of the whole machine; and although the comparison of the animal body with a machine may not be very apt, yet the organic principle is incapable of activity without this external impulse and

> without the constant material changes effected by the aid of the external vital stimuli. Richerand has therefore, not unaptly, compared the manifestation of life with the phenomenon of combustion and flame".[65]

This line of thought took him close to the idea that the vital powers and their manifestations are attributable ultimately to the chemical changes, and the releases of chemical power, which occur continuously during life; he did not get quite so far as to propose this explicitly, as Liebig the chemist was to do a little later. The closest Müller got to the chemical source of vital powers was in his discussion of animal excretions:

> "As these excretions are constant, even when the supply of nutriment is stopped, it necessarily follows that a constant decomposition of the substance of the body is essentially connected with life. It cannot, indeed, be otherwise if it be true, as it has already been proved to be, that the vital force is manifested in an animal body only while certain vital stimuli produce in the living tissues constant material changes, of which the phenomena of life are merely the external signs, just as flame is the appearance resulting from the material changes, of which the phenomena of life are merely the external signs, just as flame is the appearance resulting from the material changes effected in combustion".[66]

Further resemblence between such ideas and those that Liebig was to enunciate, was Müller's assertion of the importance of respiration in his scheme, namely that the impulse for these material changes is given by respiration.[67] Nine years later, in *Animal chemistry*, Liebig was to make his pivotal declaration that "respiration is the bent spring which keeps the clock /i.e. the living animal/ in motion".[68] Liebig was not to acknowledge any debt to Müller, and it has yet to be shown by historians of 19th century physiological chemistry whether there was any such debt. However, we may assert that, as illustrated above, Müller and Liebig held remarkably similar views on the dynamics of the living organism, declaring themselves explicitly against the possibility of its being a creator of its own power, and seeking its dynamical nutriment in the inorganic powers of the world around it. It is, moreover, inconceivable that Liebig did not read Müller's *Handbuch*; no physiologist or organic chemist in Germany during that period could have ignored Müller's work. Two likely explanations for Liebig's silence are that he was genuinely developing his own theory of dynamical physiology largely independently of Müller, and did not see why he should run the risk of allowing someone else the limelight, (for Liebig loved the limelight dearly); secondly, Liebig claimed to eschew the metaphysics of his German predecessors and proclaimed himself to be nothing but an experimental chemist; Müller was probably too much of a philosopher for Liebig to feel comfortable in acknowledging any real kinship between them. On the other hand, Müller readily acknowledged that kinship; he held back the fourth edition of his *Handbuch* long enough to incorporate certain ideas from *Animal chemistry* which appeared in the spring of 1842, commenting that Liebig had provided profound insights into the relation between respiration and nutrition. In fact, Müller's discussion of this relation was rather longer than in his previous editions, for he emphasized Liebig's view of the proportionalities between food, heat, motion and the carbon that is burnt in respiration, and Liebig's ideas on the roles of nitrogenous and non-nitrogenious nutrients. Müller did not accept Liebig's work without reservations but, as Frederick Holmes has argued very plausibly,[69] he did realize that it would henceforth set the pattern for physiology and that it could not be simply incorporated into the more traditional pattern that he himself had fashioned; henceforth, physiology would have to be based upon ~~skilful~~ skilful research in chemistry and physics; and Müller, being neither a chemist nor a physicist, withdrew from physiological investigation, refused to publish further editions of his *Handbuch* and spent the rest of his life working on anatomy. As Karl Rothschuh wrote in his *Geschichte der Physiologie* (1953),[70] Müller was more a morphologist than an experimentalist and viewed with distaste the direction in which physiological research was going - especially the use of vivisection that French physiologists were championing. Indeed, it seems that the theme of his inaugural lecture at Bonn, '*Von den Bedürfnis der Physiologie nach einer philosophischen Naturbetrachtung*' (On the need for a philosophical contemplation of Nature in physiology),[71] given in 1824, remained the *leitmotif* throughout his career and explains why he was becoming more and more estranged from experimental physiology from the late 1830s onwards. Nonetheless, his influence persisted into the 1840s, for the *Handbuch* had deliberately raised many key questions and had left them unanswered. One of those questions concerned the source of organic forces, to which a handful of Müller's most brilliant pupils and the chemist, Liebig, addressed themselves.

NOTES TO CHAPTER 13.

1. M. Müller, '*Ueber die philosophischen Anschauungen des Naturforschers Johannes Müller*', *Arch.Gesch.Med.*, 1926, 18:131-132.

2. Cited by J. Steudel, 'Johannes Peter Müller', in C.G. Gillispie (editor), *Dictionary of scientific biography*, New York, Charles Scribner's Sons, 1974, ix:568.

3. J. Müller, 'On the life and writings of the late Professor Rudolphi', *Edinb.New Phil.J.*, 1838, 25:227.

4. J. Müller, *Zur vergleichenden Physiologie des Gesichtssinnes des Menschen und der Thiere*, Leipzig, C. Crobloch, 1826.

5. *Ibid.*, pp.44-45.

6. J. Müller, *Ueber die phantastischen Gesichtserscheinungen*, Coblenz, J. Hölscher, 1826.

7. *Ibid.*, pp. iii-iv.

8. *Ibid.*, p.v.

9. *Cf.*V. Kruta, *J.E. Purkyně (1787-1869), physiologist*, Prague, Academia Publishing House of the Czech Academy of Science, 1969.

10. Wundt was a colleague of Helmholtz for a while in Heidelberg before taking up a chair in Zürich.

11. J.T. Merz, *A history of european thought in the nineteenth century*, New York, Dover, 1965, vol.2, pp.483-484.

12. W. Wundt, *Grundzüge der physiologischen Psychologie*, Leipzig, W. Engelmann, 1887.

13. *Cf.* Helmholtz, *op.cit.*, Note 54 below.

14. Müller cites him in *op.cit.*, Note 4 above, pp.61-62.

15. F. Magendie, '*Le nerf olfactif est il l'organe de l'odorat? Expériences sur cette question*', *J.Physiol.*, 1824, 4:170.

16. W.R. Woodward, 'Hermann Lotze's critique of Johannes Müller's doctrine of specific sense energies', *Med.Hist.*, 1975, 19:149.

17. J. W. von Goethe, *Theory of colours*, trans. by C.L. Eastlake, Cambridge,Mass., M.I.T. Press, 1970. There is a useful discussion of the relationship between Goethe and Purkyně, and of Goethe's colour theory, in V. Kruta, *The poet and the scientist, Johann Wolfgang Goethe and Jan Evangelista Purkyně*, trans. by L. Pantučková, Prague, Academia, 1968.

18. Müller, *op.cit.*, Note 4 above, pp.395-396.

19. J. Müller, '*Ueber das Bedürfnis der Physiologie nach einer philosophischen Naturbetrachtung*', *Antrittsvorlesung, 19.Okt., 1824*, reprinted as chapter 1, Müller, *op.cit.*, Note 4 above, pp.27-28.

20. *Ibid.*, p.26.

21. *Op.cit.*, Note 15 above.

22. Müller, *op.cit.*, Note 6 above, p.23.

23. J.W. von Goethe, *Versuch die Metamorphose der Pflanzen zu erklären*, Gotha, C.W. Ettinger, 1790.

24. *Op.cit.*, Note 6 above, p.vii.

25. There is an invaluable account of Müller and his school in K.E. Rothschuh, *History of physiology*, trans. by G.B. Risse, New York, Robert E. Krieger, 1973, pp.195-204. Also see Everett Mendelsohn, 'Explanation in nineteenth century biology', in R.S. Cohen and M.W. Wartofsky (editors) *Boston studies in the philosophy of science*, New York, Humanities Press, 1965, pp.127-155.

26. Cited in Wheeler, *op.cit.*, Note 29 below, p.62.

27. H. Driesch, *History and theory of vitalism*, trans. by C.K. Ogden, London, Macmillan, 1914, pp.113-118 particularly.

28. H. Driesch, *Science and philosophy of the organism*, London, A. & C. Black, 1929, pp.223-225 particularly.

29. L. Richmond Wheeler, *Vitalism: its history and validity*, London, H.F. & G. Witherby, 1939, pp.62-64 particularly.

30. O. Temkin, 'Materialism in French and German physiology of the early nineteenth century', *Bull.Hist.Med.*, 1946, 20:322-327. Also Temkin, 'The dependence of medicine upon basic scientific thought', in McC. Brooks & P.F. Cranefield (editors), *The historical development of physiological thought*, New York, Hafner Publishing Co., 1959, pp.5-21.

31. G. Koller, *Das Leben des Biologen Johannes Müller*, Stuttgart, Wissenschaftliche Verlagsgesellschaft, 1958.

32. W. Riese and G.E. Arrington, 'The history of Johannes Müller's doctrine of the specific energies of the senses: original and later versions', *Bull.Hist.Med.*, 1963, 37: 179-183.

33. *Op.cit.*, Note 25 above.

34. E. Mach, *The analysis of sensations*, trans. by C.M. Williams and supplemented from the 5th German edition of 1906, New York, Dover, 1959. See particularly pp.1, 27, 59, 122-126.

35. *Op.cit.*, Note 25 above, p.135.

36. *Ibid.*, pp.135 ff.

37. J. Müller, *Elements of physiology*, trans. by W. Baly, London, Taylor & Walton, 1838, pp.612-613.

38. *Ibid.*, pp.622-633.

39. A Monro & R. Fowler, *Abhandlungen über thierische Electricität und ihren Einfluss auf das nervensystem*, Leipzig, Weygandsche Buchhandlung, 1796. Müller also cites Fowler's original English treatise on animal electricity of 1793.

40. *Op.cit.*, Note 37 above, p.624.

41. Helmholtz, *op.cit.*, Chapter 16, Note 21 below.

42. *Op.cit.*, Note 37 above, from the lengthy and important 'Prolegomena', p.57.

43. *Ibid.*, p.58.

44. *Ibid.*, pp.26 ff, particularly pp.49, 51, 53.

45. *Ibid.*,p.50.

46. *Ibid.*, p.1342.

47. *Ibid.*, p.1339.

48. *Ibid.*, pp.1341-1342.

49. *Ibid.*, pp.26-27

50. *Ibid.*, p.27.

51. Cited by V. Kruta, in C.G. Gillispie (editor), *Dictionary of scientific biography*, New York, Charles Scribner's Sons, 1971, iv:265.

52. Leo Koenigsberger, *Hermann von Helmholtz*, Braunschweig, F. Vieweg und Sohn, erster Band 1902, zweiter Band 1903, dritter Band 1903.

53. H. Helmholtz, *Treatise of physiological optics*, edited by J. Southall, trans. from the 3rd German edition, Menasha,Wisc., G. Banta, 1924, vol.3, section on 'Historical and critical survey'.

54. Cited in Russell Kahl, *The selected writings of Hermann von Helmholtz*, Middletown, Conn., Wesleyan Univ.Press, 1971, p.xxiii.

55. *Ibid.*, p.xxiii.

56. Müller, *op.cit.*, Note 37 above, p.1068.

57. *Ibid.*, p.1068.

58. *Ibid.*, pp.1059-1087.

59. *Ibid.*, pp.1333-1420.

60. Aristotle, *De anima*, 403 b.20-405 b.31.

61. *Ibid.*, 404 b.8-17.

62. Müller, *op.cit.*, Note 37 above, p.1358.

63. *Ibid.*, pp.50-51.

64. Aristotle, *De anima*, 422 b.18-423 a.2.

65. Müller, *op.cit.*, Note 37 above, pp.29-30.

66. *Ibid.*, p.38.

67. *Ibid.*, p.38.

68. J. Liebig, *Animal chemistry*, New York, Johnson Reprint Corp., 1964, p.27.

69. F.L. Holmes in his introduction to *ibid.*, pp.lxix-lxx.

70. Rothschuh, *op.cit.*, Note 25 above, p.202.

71. Müller, *op.cit.*, Note 19 above.

CHAPTER 14. On power in the physiological chemistry of Justus Liebig (1803-73)

Since there are several excellent accounts readily available in English and German of Liebig's life and work I shall give hardly any biographical details here, except to mention a few essential features of his university education and career. He received his medical education first at Bonn, then at Erlangen. At Bonn, one of his fellow students was Johnanes Müller, a fact of some interest because of the similarity which was to arise in some of their key ideas. Exactly what their relationship was, if any, during their student or later years has not yet been elucidated; it is quite probable that they never met during their student period, although they would have been exposed to the same teachers, amongst whom were several ardent *Naturphilosophen*. (See the introduction to the previous chapter.) Yet even if they did not become acquainted as students, it is inconceivable that Liebig was not to read Müller's *Handbuch der Physiologie* during the 1830s, and it is most probable that he well appreciated the general direction and import of Müller's physiological dynamics by the time he wrote his own treatise on physiological chemistry.

In 1822 Liebig went to Paris to study under the brilliant French chemists who were developing new techniques in quantitative analysis. There he caught the eye of Alexander von Humboldt who secured him a place in the laboratory of Louis Joseph Gay-Lussac (1778-1850) where he spent about a year. In 1824, again at von Humboldt's recommendation, he returned to his home country, the grand-duchy of Hesse-Darmstadt, to take up an appointment as professor-extraordinary of chemistry in the small university of Giessen. Over the next fifteen years he built up a thriving research and teaching chemistry department, extended the analytical techniques he had learnt in Paris and achieved his ambition of putting Germany on the map as a leading country in the newly-rigorous science of chemistry. To Giessen, students flocked from all over Europe and North America, and by the time Liebig left to take up a chair in Munich in 1852, he had become the world's most renowned, or at least most vocal and controversial, organic chemist.

Liebig's contribution to the emergence of the Conservation of Energy has been analysed by several scholars. In his 1957 paper entitled 'Energy conservation as an example of simultaneous discovery',[1] Thomas Kuhn recognized Liebig as one of a dozen or so discoverers of the principle. Kuhn's paper was a superbly written and truly scholarly piece of historical research, and it may be justly regarded as a classic in this field. However, there is one important point in his paper on which I take issue, namely the way whereby Liebig was supposed to have arrived at his own realization of energy conservation. According to Kuhn, he had been considering the duty of electric motors, as a result of which he proposed that the chemical equivalents of the elements involved determine the work that is retrievable from chemical processes by either electrical or thermal means. In Kuhn's words:

> "Joule and Liebig reached energy conservation by asking an old engineering question, 'What is the duty?' about the new conversion processes in the battery-driven electric motor".[2]

It seems to me that Kuhn, as later Liebig scholars have done, focussed largely on Liebig's *Die Thierchemie* (1842),[3] which did discuss the duty of the electric motor, and his *Chemische Briefe*[4] first published in 1844 and translated that year into English as *Familiar letters on chemistry*. What Kuhn apparently did not do was examine Liebig's earlier treatise, *Die Chemie in ihrer Anwendung auf Agrikultur und Physiologie* (1840),[5] (henceforth to be called *Agricultural chemistry*.) Other scholars have followed Kuhn's disregard of that work; for instance, Frederick Holmes in his fine introduction to the 1964 reprint of the first English edition of *Animal chemistry* discussed how it gave Helmholtz his cue for investigating forces in a physiological context,[6] but he did not mention that the *Agricultural chemistry* contained many of the ideas, including those on forces, which were to appear in the more famous *Animal chemistry*.

To appreciate the connection between those two treatises, let us recall that in 1837 the British Association for the Advancement of Science asked Liebig, as one of the leading organic chemists in Europe, to compile a report on the state of organic chemistry. (It was a frequent practice of the British Association in its early years to commission comprehensive reports from experts on their own sciences:[7] to wit, William Clark's 'Report on animal physiology' mentioned earlier.) *Animal chemistry* was the second part of that report; the *Agricultural chemistry* was the first part, and it therefore set the tone, and contained many of the ideas, of the second.

It seems to me that these two parts of Liebig's report on organic chemistry together constituted, among other things, the detailed enunciation of a new principle, which Liebig himself believed to be important and of universal applicability: that principle was the conservation of energy or force, (Liebig did not always distinguish between these two concepts often used *Kraft* for both of them). Moreover, since Liebig was enunciating that principle within the context of organic chemistry and physiology, Kuhn's assertion of the electric-motor origin of his principle must be queried.

In the following pages, Liebig's interest in power or force will be traced in his 1840 and 1842 treatises and in his *Familiar letters on chemistry*.

Throughout the first part of his *Agricultural chemistry* he showed a concern for the transformations of inorganic into organic matter; these required forces, not only chemical affinities, but also heat, motion, light and electric force. Moreover, these forces could themselves undergo transformation, chemical forces producing electrical ones and heat, sunlight producing chemical forces in green parts of plants, etc.

Liebig was a vitalist, but his faith in the applicability of chemistry to organic processes resulted in his vital force being a carefully defined and somewhat restricted one. Essentially, vital force was for him, as it was for Blumenbach, a device to explain the wonderful process of generation:

"Vitality is the power which each organ possesses of constantly reproducing itself; for this, it requires a supply of substances which contain the constituent elements of its own substance, and are capable of undergoing transformation".[8]

Such transformations were, in themselves, purely chemical processes. Between these two polarities of organic activities, that is, between the power of vitality and the powers of chemistry, Liebig envisaged a world where these powers mingled, cooperated, opposed each other and usually ended up in a sort of unstable equilibrium; power was his paradigm, and the two extreme types were the vital and the chemical, between which there was often an unholy alliance. Liebig's vital principle was a subtle agent which is difficult for the historian to define. Indeed some of his writings seem to deny a vital force, and even his contemporaries were not always sure of his opinion. Perhaps the most felicitous account that he himself gave is the following from *Agricultural chemistry*, where he suggests that *Lebenskraft*, though real, is not a useful tool for research, and that the physiological chemist must work as if the organism obeys purely physico-chemical laws:

"We should not permit ourselves to be withheld, by the idea of a vital principle, from considering in a chemical point of view the process of the transformation of the food, and its assimilation by the various organs. This is the more necessary, as the views hitherto held have produced no results, and are quite incapable of useful application".[9]

A particularly topical issue at that time which involved the debate between vitalism and mechanicism was the question of digestion. Despite the study of it by Gmelin and Tiedemann, some physiologists still considered it to be an essentially vital process. Then in the 1830s Theodore Schwann and Johann Nepomuk Eberle (? - 1834) furnished further evidence for the purely chemical point of view, and with their results behind him Liebig denied, in the *Agricultural chemistry*, that the digestive powers were vital. However, realizing the complexity of the issue, he felt compelled to warn that the vital force had many points of contact with chemical forces, that the latter could even seem to replace it for certain functions, and that it was therefore incumbent on physiologists to investigate those points of contact and not to champion vitalism to the total neglect of chemical forces, or *vice versa*.[10] That investigation, in my opinion, is precisely what he attempted in *Animal chemistry* two years later.

One of the most convincing arguments for the existence of vital force for Liebig seems to have been simply the apparent magnitude of its power, compared with the power of inorganic forces. One could see this in the amazing productivities of certain plants,

> "... to which the most powerful chemical action cannot be compared. The best idea of it may be formed, by considering that it surpasses in power the strongest galvanic battery, with which we are not able to separate the oxygen from carbonic acid".[11]

This interest in the immense power of vital force reappeared in *Animal chemistry* and the *Familiar letters on chemistry*, where it was sometimes linked with a discussion of the duties of various engines. Such passages are the ones that Kuhn had in mind.

Despite the sheer power and apparent versatility of vital force, Liebig attempted to explain a host of hitherto enigmatic and apparently wholly vital phenomena in terms of physico-chemical powers and the motions they could induce; fermentation, putrefaction and decay could be extracted from his physico-chemical crucible, wherein vital force as an efficient cause had barely any rôle. His key idea in these accounts was that an agent, such as a ferment, possessed internal movement or vibrations, and that when placed among molecules of a substrate, such as sugar, it could communicate its vibration to them, thus causing them to be shaken apart into simpler molecules, such as alcohol and carbonic acid. This hypothesis sounds crude to us today, and there were indeed many objections raised against it in Liebig's own time; yet he was able to call upon a fair amount of experimental evidence for support, particularly work done by French investigators. One of the most interesting passages where he explained the idea occurs in the section on poisons: one type of poison was supposed to exert a purely chemical effect on the body, by combining with components of the body itself; there was another type, however, that acted

> "... not on account of their entering into combination with it, or by reason of their containing a poisonous material, but solely by virtue of their peculiar condition.
>
> In order to attain to a clear conception of the mode of action of these bodies, it is necessary to call to mind the cause on which we have shown the phenomena of fermentation, decay and putrefaction to depend.

> This cause may be expressed by the following law, long since proposed by Laplace and Berthollet, although its truth with regard to chemical phenomena has only lately been proved. 'A molecule set in motion by any power can impart its own motion to another molecule with which it may be in contact'.
>
> This is a law of dynamics, the operation of which is manifest in all cases, in which the resistance (force, affinity or cohesion) opposed to the motion is not sufficient to overcome it.
>
> We have seen that ferment or yeast is a body in the state of decomposition, the atoms of which consequently are in a state of motion or transposition. Yeast placed in contact with sugar communicates to the elements of that compound the same state, in consequence of which the constituents of the sugar arrange themselves into new and simpler forms, namely into alcohol and carbonic acid. In these new compounds, the elements are united together by stronger affinities than they were in the sugar ...."[12]

To this he added that the idea of yeast reproducing itself as seeds reproduce seeds, that is in the manner of living organisms, was absurd. This hypothesis had been championed recently by three able investigators - Charles Cagniard-Latour (1777-1859), Theodore Schwann, and Friedrich Kützing (1807-1893). In 1839, Berzelius, then the most influential chemist in Europe and still on friendly terms with Liebig, had reviewed Kützing's work; he admitted that it might have some value as a microscopical study, but the idea that fermentation could be due to living organisms seemed to him to be much too metaphysical and inimical to the progress of science.[13] In Liebig's *Annalen* of that year, Liebig and Wöhler followed Berzelius in rejecting this hypothesis; their article was in bad taste and vituperative, for Liebig regarded the hypothesis as naïve and arrant vitalism. This was in keeping with his view, mentioned above, that the transformations that occur in organic systems are purely chemical, and that to invoke primarily a vital force, let alone a whole organism as Schwann, Cagniard-Latour and Kützing had done, was wholly erroneous. Indeed, Liebig was able to explain simple processes of reproduction, of which the growth of a mass of yeast seemed to him to be one example, in purely chemical terms:

> If a body A,e.g., oxamide,be brought into contact with another compound B, which is to be reproduced; and if this second body be oxalic acid dissolved in water, then ... the oxamide is dissolved by the oxalic acid, provided the conditions necessary for their exercising an action upon one another are present. The elements of water unite with the constituents of oxamide, and ammonia is one product formed, and oxalic acid the other ....
>
> If we now add to the same mixture a fresh portion of oxamide, the same decomposition is repeated; ... in this manner, a very minute quantity of oxalic acid may be made to effect the decomposition of several hundred pounds of oxamide; and one grain of the acid to reproduce itself in unlimited quantity".[14]

We see here that Liebig had cracked one problem that had bedevilled the study of generation and which, as discussed in my earlier Chapter , Blumenbach and others had resolved only by employing dynamical agents: namely, the problem of explaining how living organisms can share out, or pass on, their powers and other properties to their offspring, without those powers and properties undergoing any diminution in successive generations. Liebig's solution lay in chemical forces, for each new molecule of oxalic acid (or yeast) derived its powers and properties from a preceding molecule of oxamide (or sugar); the powers that appeared in each new molecule of oxalic acid (or yeast) were therefore purely chemical powers which had existed, albeit in different conditions and therefore with different manifestations, in the nutritive molecules.

Nonetheless, Liebig did not pretend to have solved the entire mystery of generation and he therefore still needed the vital force:

> "Our notion of life involves something more than mere reproduction, namely the idea of an active power exercised *by virtue of a definite form*, and production and generation *in a definite form*. By chemical agency we can produce the constituents of muscular fibre, skin and hair; but we can form by their means no organized tissue, no organic cell".[15]

The characteristic effect of vital force was therefore the forms that organic matter assumed; in Aristotelian terms, we might call Liebig's vital force a purely formal cause, and not an efficient cause, of life; in this vein, Liebig was able to envisage inorganic powers as the sole efficient agents in organic processes. Yet there was also a sense in which vital force was more than mere form, for its capacity to direct the chemical powers meant that it was similar to heat, electricity and other agents which could also regulate chemical powers. Thus:

> "The chemical forces are subordinate to this cause of life, just as they are to electricity, heat, mechanical motion, and friction. By the influence of the latter forces, they suffer changes in their direction, and increase or diminution of their intensity, or a complete cessation or reversal of their action. Such an influence, and no other, is exercised by the vital principle over the chemical forces".[16]

At this stage we might ask whether he discussed the origins of vital power, for only thus can we determine whether Liebig considered the living organism to be a *perpetuum mobile*. This issue was to be discussed at length in the *Animal chemistry*, but in the 1840 treatise it received only brief discussion: his argument was reminiscent of Müller's distinction between stimuli that exhaust the organism and true vital stimuli, and like Müller he chose respiration as the main renewer of life:

> "The vital principle opposes to the continual action of the atmosphere, moisture and temperature upon the organism, a resistance which is, in a certain degree, invincible. It is by the constant neutralization and renewal of these external influences that life and motion are maintained.
>
> The greatest wonder in the living organism is the fact that an unfathomable wisdom has made the cause of a continual decomposition or destruction, namely the support of the process of respiration, to be the means of renewing the organism, and of resisting all the other atmospheric influences".[17]

Thus, the living organism was treated as being dependent on external influences, namely the inorganic powers and respiratory supplies, for its activity; it was not a *perpetuum mobile*. As if to leave no doubt about this point, he devoted the penultimate paragraph of the treatise to it:

> "After the removal of the cause which forced their union - that is, after the extinction of life - most organic atoms retain their condition, form and nature, only by a *vis inertiae*; for a great law of nature proves that matter does not possess the power of spontaneous action. A body in motion loses its motion only when a resistance is opposed to it; and a body at rest cannot be put in motion or any action whatever, without the operation of some exterior cause".[18]

We must be cautious in evaluating this paragraph. Liebig denied emphatically the possibility of a *perpetuum mobile*, either in living or inorganic systems; but that was not tantamount to an assertion of the Conservation of Energy; it was only an assertion of the non-creatibility of motion. However, he seemed to be reaching out for a more fundamental principle, in that he used the word 'activity' (*Thätigkeit*) in addition to 'motion' (*Bewegung*), and chose to express the universal law in the most general terms, namely, that matter is incapable of spontaneous activity. This more general and fundamental form of the denial of perpetual motion loomed large in *Animal chemistry*.

Part III of *Animal chemistry*, the last and most important part for the discussion of power in the animal economy, propounds the conservation of motion often accompanied by expressions, some vague but others quite precise, of the conservation of forces within the organism. Actually, he usually expressed this latter idea as conservation of the momentum of force (*Kraftmoment*), meaning the work that a moving force could do. (This is one occasion when the original words of the author are so important and difficult to translate exactly that they must be quoted in German):

> "*Die Wirkung ist folglich nicht der bewegenden Kraft allein, noch der Zeit allein, sondern dem Druck, multipliciert mit der Zeit = Kraftmoment, proportional*".[19]

This seems to have been one of Liebig's several attempts to differentiate between what we today call force on the one hand, and work or energy on the other. A more rigorous attempt at such differentiation occurred a few pages later:

> "*Wir wissen, dass dieses Bewegungsmoment der Lebenskraft in einem belebten Körpertheil verwendbar ist, um ruhenden Materien Bewegung zu ertheilen (Zersetzung zu bewirken, Widerstände aufzuheben), und wenn die Lebenskraft in ihren Aeuserungen sich ähnlich verhält wie andere Kräfte, so muss dieses Bewegungsmoment mitgetheilt oder fortgepflanzt werden können durch Materien, die in sich selbst durch eine entgegenwirkende Thätigkeit seine freie Aeuserung nicht aufheben*".[20]
>
> ("We know that this momentum of motion in the vital force, residing in a living part, may be employed in giving motion to bodies at rest (that is, in causing decomposition, or overcoming resistance), and if the vital force resembles other forces in its manifestations, this momentum of force must be able to be conveyed or communicated by material bodies, which in themselves do not destroy its effect by an opposite manifestation of force".)

On motion itself, he asserted that by whatever cause produced, it cannot be annihilated; it might become inappreciable to human sense, but even when arrested by a resisting force, its effect is not annihilated.[21] This was all totally in keeping with Des Cartes and Leibniz (although, as usual, Liebig did not cite anyone); where he was original was in applying the principle pre-eminently to physiology.

Liebig suggested that the usual conceptions of motion, equilibrium and resistance could be transferred to chemical forces and thence to vital force, for it was his belief that the *modus operandi* of vital force was infinitely closer to that of chemical forces, than to any other type of force. This suggestion was developed greatly in Part III. For instance, in explicating "the phenomena of motion in the animal organism", he began by considering the voltaic cell, in which the rôle of chemical force was obvious: chemical activity produced the manifestation of electrical force in a wire connecting a series of zinc and copper plates dipped in an acid, and it was to be noted particularly that the wire itself did not produce this force. Even more important from the point of view of this thesis is that Liebig did not indulge in any *Naturphilosophie*-type speculation about the way whereby the original chemical force becomes transformed into other types of force (heat, electricity or magnetism) during its passage through the wire. In Chapter 10 we saw how Oersted had concerned himself with such a question. But not Liebig. In fact, he went so far as to say:

> "In the preceding paragraphs we have considered these remarkable phenomena in a form which is independent of the explanations of the schools. ... All the suppositions which may be employed as explanations of the phenomena have not the slightest influence on the truth of these phenomena; for they refer merely to the form in which they are manifested".[22]

All he would say was that the chemical forces of the voltaic cell had produced a momentum of motion which in turn produced mechanical effects. Similarly, momenta of motion were produced in the animal organism, the nerves being the conveyors of vital force to the muscles. In this way he could explain those vital phenomena that had been explicated by the old theory of sympathy (see my chapter on Cullen and Brown), although he typically did not refer to that theory or any of its exponents.

The generator of force for motion in the animal was muscular tissue, according to Liebig; the rôle of vital force was to transmit the moving forces from one limb to another and to induce a chemical change in the recipient tissue. This latter lost some of its vitality as a result of its chemical change, since as he had explained in the *Agricultural chemistry*, vital force was a consequence or phenomenon of particular forms of matter which depended upon particular chemical compositions. (It was essentially Reil's theory). In his own words:

> "A living part acquires, on the above supposition, the capacity of offering and overcoming resistance, by the combination of its elementary particles in a certain

> form; and as long as its form and composition are not destroyed by opposing forces, it must retain its force uninterrupted and unimpaired".[23]

Since any change in muscular composition and form entailed a concomitant diminution in its stock of vital force, there immediately arose an imbalance between the chemical forces inherent in the tissue's composition and its vital force; since chemical forces, especially those possessed by oxygen in the blood circulation, tended continuously to break down or oxidize the living tissues, that portion of muscle which had lost its vitality would be speedily oxidized. The net process was that the change of form of the muscle tissue generated mechanical force, just as the chemical changes in the voltaic cell generated mechanical force; moreover, since vital force initiated the process, vital force had generated an equivalent quantity of mechanical force.[24]

This synopsis of Liebig's theory of animal motion differs slightly, but not importantly, from what other historians would say; see, for instance, the fine paper on 'Vitalism and Reductionism in Liebig's physiological thought' by Timothy Lipman.[25] One aspect of Liebig's theory that Lipman does not discuss was his persistent effort to express it rigorously and quantitatively; this, as well as the considerable number of pages that Liebig gave to discussing the relations and interconversions among vital, chemical and mechanical forces, show us how crucial such dynamics were in his physiology. We can see this in the following:

> "The change of matter, the manifestation of mechanical force, and the absorption of oxygen are, in the animal body, so closely connected with each other, that we may consider the amounts of motion, and the quantity of living tissue transformed, as proportional to the quantity of oxygen inspired and consumed in a given time by the animal. For a certain amount of motion, for a certain proportion of vital force consumed as mechanical force, an equivalent of chemical force is manifested; that is, an equivalent of oxygen enters into combination with the substance of the organ which has lost the vital force; and a corresponding proportion of the substance of the organ is separated from the living tissue in the shape of an oxidized compound".[26]

So we see that Liebig was greatly concerned with proportionality between forces or causes, and their manifestations, momenta of motion or effects. His most unequivocal expressions of this concern occurred in Part I of *Animal chemistry*, particularly Sections IV and V,[27] which I consider to be one of the lynch-pins of the whole treatise. (No other commentators on Liebig, to my knowledge, have considered these sections to be so important; this is probably because they have not been as exclusively concerned with his dynamics as I am. Timothy Lipman, however, has made some perceptive comments on his physiological dynamics, which will be mentioned below.) At the end of Section IV, in discussing respiration, Liebig developed several mechanistic analogies for the rôle of that function in the animal; one was the flame, which is extinguished when its oil is consumed by the atmospheric oxygen; another was the pendulum-clock, where we see how crucial he considered respiration to be in the regulation of the dynamics of the animal economy; in reading this passage, we might recall what Müller had said about respiration, although Liebig did not acknowledge him or anyone else for his idea:

> "Respiration is the falling weight, the bent spring, which keeps the clock in motion; the inspirations and the expirations are the strokes of the pendulum which regulate it".[28]

He asserted that just as the effects of the length of the pendulum and the temperature of the air on ordinary time-pieces were known already with mathematical accuracy, so would be the physiological effects of the air and temperature. That these were envisaged mainly as dynamical effects, rather than material ones, and that Liebig envisaged the organism as a dynamical economy, rather than a material structure, are evident in his next paragraph - the start of Section V:

> "The want of a just conception of force and effect and the connexion of natural phenomena has led chemists to attribute a part of the heat generated in the animal body to the action of the nervous system. If this view exclude chemical action, or changes in the arrangement of the elementary particles, as a condition of nervous agency, it means nothing else than to derive the presence of motion, the manifestation of a force, from nothing. But no force, no power, can come from nothing".[29]

Liebig was in no doubt that heat is a true dynamical agent[30] and spent the rest of Section V discussing its production from, and correlations with, other forces: by the combustion of carbon, by solution of a metal in an acid, by the combination of the two electricities, by absorption of light and by mere friction. Then, again not mentioning his predecessors, he discussed the duty of the steam-engine:

> "When we kindle a fire under a steam-engine, and employ the power obtained to produce heat by friction, it is impossible that the heat thus obtained can ever be greater than that which was required to heat the boiler; and if we use the galvanic current to produce heat, the amount of heat obtained is never, in any circumstances, greater than we might have by the combustion of the zinc which has been dissolved in the acid".[31]

Perhaps he did not refer to other theorists on the steam-engine because the principle was already quite well-known; moreover, as Thomas Kuhn points out,[32] those theorists who had enunciated a rigorous relation between heat and work, namely Sadi Carnot before 1832 and Marc Séguin in 1839, had not been able to develop it into a general principle for all forces; and other workers in the energy conservation field usually did not realize the general import of those heat studies. Consequently, Liebig probably did not consider those studies on heat and work important or broad enough to merit acknowledgement. Nonetheless, heat and its dynamical correlations were important for his physiology; they led to the important assertion, which occurred only once or twice in *Animal chemistry* so explicitly, that the ultimate cause of muscular motion was chemical force - the implication of which was that even the vital force, which was transmitted to muscles to induce them to act, was ultimately one with chemical force. As he put it, immediately after his discussion on the steam-engine and the galvanic current:

> "The contraction of muscles produces heat; but the force necessary for the contraction has manifested itself through the organs of motion, in which it has been excited by chemical changes. The ultimate cause of the heat produced is, therefore, to be found in these chemical changes".[33]

As if to emphasize the right of admission of the vital force to the pantheon of powers in Nature, Liebig continually juxtaposed his discussions of vital force with discussions of inorganic forces. As Lipman mentions,[34] he was confident enough in the reality of vital force not to undertake a major defence of it in his physiology, but he did feel the need to draw attention to its profound similarities with the forces of natural philosophy. Thus, in the very first paragraph of Part III, he declared that if ever it should be proved that the vital force, in its manifestations, had nothing in common with those forces which were known to produce motion or change in inorganic nature, all his arguments on animal motion (which constituted a huge chunk of his physiology) would be invalidated.[35] Near the beginning of the book he had discussed briefly the ontology of forces and concluded that, since all other forces could be inferred solely from their effects, and since there seemed to be one range of effects or phenomena in Nature which diverged from all other phenomena, it was perfectly philosophical to infer the existence of a different, a vital, force. It is tempting for historians of Liebig to sniff out metaphysical strands in these ideas; indeed, any German natural philosopher of that period who claimed as vehemently as Liebig did to be free from metaphysics automatically arouses suspicion. As Ernst von Meyer in his *Geschichte der Chemie* (1888)[36] and J.T. Merz in his *European thought in the nineteenth century* (1923-1950)[37] pointed out, Liebig's two years' study under Schelling provided him with a philosophy of vitalism that he never surrendered. Actually, Liebig himself admitted at least a temporary infatuation with *Naturphilosophie*, in an essay entitled *Ueber das Studium der Naturwissenschaften* (On the study of the natural sciences) published in 1840:

> "I myself spent a portion of my student days at a university where the greatest philosopher and metaphysician of the century charmed the thoughtful youth around him into admiration and imitation; who could at that time resist the contagion? I too have lived through this period - a period so rich in words and ideas and so poor in true knowledge and genuine studies; it cost me two precious years of my life".[38]

Liebig was remarkably unwilling throughout his life to discuss philosophical issues and it is, as yet, impossible to determine any lasting impact that Schelling and other philosophers might have made on him. Perhaps a close study of his friendship with Graf August von Platen-Hallermünde (1796-1835) the famous poet and essayist, who seems to have "fallen in love with him" at first sight when they met in 1822 and with whom he remained friendly until von Platen's death, might reveal something.[39]

For those of us who hope to discover metaphysical skeletons, particularly of a *naturphilosophische* frame, in Liebig's cerebral cupboard, even his discussions on the ontology of force are disappointing. Although there is obvious similarity between them and Kant's philosophy of knowledge, to wit the distinction between noumena and phenomena, it was not only Kant who informed his discussion, but also that well-known and oft-regurgitated declaration by Newton when he had had to defend his force of gravitation against Cartesians and other critics; as Liebig wrote:

> "Natural science has fixed limits which cannot be passed; and it must always be borne in mind that, with all our discoveries, we shall never know what light, electricity and magnetism are in their essence, because even of those things which are material, the human intellect has only conceptions. We can ascertain, however, the laws which regulate their motion and rest, because these are manifested in phenomena. In like manner, the laws of vitality and of all that disturbs, promotes or alters it may be discovered, although we shall never know what life is. Thus, the discovery of the laws of gravitation and of the planetary motions led to an entirely new conception of the cause of these phenomena. This conception could not have been formed in all its clearness without a knowledge of the phenomena out of which it was evolved; for considered by itself, gravity, like light to one born blind, is a mere word, devoid of meaning".[40]

The laws of vitality, he believed, would be found in harmony with the universal laws of motion and force "which preserve in their courses the worlds of our own and other systems".[41] Perhaps the most important law of vitality that Liebig considered himself to have discovered, was that every action of the living organism necessitates a transformation of some part of itself into unorganized matter; that therefore every motion, every manifestation of force, is the result of a chemical change; that even every conception, every mental affection, every sensation, is accompanied by a change of composition of the brain;[42] and that at the root of all these processes lies the utilization of chemical force which alone is the fuel keeping the organism going. As he wrote early on in *Animal chemistry*:

> "In order to keep up the phenomena of life in animals, certain matters are required, parts of organisms, which we call nourishment. In consequence of a series of alterations, they serve either for the increase of the mass (nutrition), or for the supply of the matter consumed (reproduction), or finally for the production of force."

And:

> "If the first condition of animal life be the assimilation of what is commonly called nourishment, the second is a continual absorption of oxygen from the atmosphere ....
>
> All vital activity arises from the mutual action of the oxygen of the atmosphere and the elements of the food.
>
> In the processes of nutrition and reproduction, we perceive the passage of matter from the state of motion to that of rest (static equilibrium); under the influence of the nervous system, this matter enters again into a state of motion. The ultimate causes of these different conditions of the vital force are chemical forces".[43]

As these passages show, and as Lipman has argued, Liebig considered his vital force to be a thoroughly respectable and scientific agent. Far from using it to set aside the living organism as a piece of mechanism distinct from all others in its capacity to generate force, he was at pains to correlate it with all the other dynamical agents in Nature. The organism thus became a powerhouse in which the momentum of force, or power output, could never be greater than the power input; if output and input were balanced perfectly, good health and physiological equilibrium resulted; if input exceeded output, growth resulted. In this, Liebig's theory was much akin to those of Hufeland, Blumenbach and various of the British physiologists already mentioned.

Turning now to his more populist publication, *Familiar letters on chemistry*, which at least one late 19th century British encyclopaedia called his most memorable work, we find several letters devoted largely to force or power. Although the first edition of that work appeared in 1844, that is, a few years after Grove, Joule and Mayer had first announced their ideas on force, Liebig almost certainly did not owe anything to them; almost certainly, he did not know of Grove's and Joule's studies, and although Mayer had published his theory in Liebig's own *Annalen*, it is unlikely, as Thomas Kuhn points out,[44] that Liebig realized its import at that time. Therefore, what he wrote about the connections of forces in the first edition of *Familar letters* were probably his own ideas; they resembled closely what he had written in *Agricultural* and *Animal chemistry*.

Letter VII, 'On mechanical forces', was at pains to arrange the vital force alongside the other forces of Nature;

vital force was simply another force, about which there was no need to make any fuss. Liebig was simply continuing a theme which, as we have seen, he had discussed in 1842. Thus:

> "Light, Heat, the Vital Principle, and the Force of Gravity exercise a most decided influence upon the number of the simple atoms which unite to form a compound atom, and upon the manner of their arrangement. They determine the form, properties, the characteristic qualities of the combinations, precisely because they are able to communicate motion to atoms at rest, and to annihilate motion by resistance.
>
> Light, heat, the vital principle, the electric and magnetic forces, the power of gravity, manifest themselves as forces of motion and of resistance, and as such change the direction and very the strength of the chemical force".[45]

Indeed, if any force was special it seemed to be chemical force!

Letter VIII, 'The vital principle', dealt explicitly with the connections among forces. It asserted the intimacy of the interactions of the vital force with all other forces, re-emphasized its similarity to those forces and merely repeated the arguments that had been used in the earlier treatises.

Letter IX, 'Transformations of almond milk', discussed the vitalism-mechanicism issue with particular regard to the various hypotheses on fermentation; no new arguments on vital force or other forces were advanced, but what was surprising was his declaration that "... the greatest and most enduring acquisition which chemical science has derived from the study of fermentation" was the principle of Laplace and Berthollet, that an atom or molecule put in motion by any power whatever would communicate its own motion to any atoms in contact with it.

Throughout this edition, Liebig seemed to declare that his general purpose had been not only to discuss the main issues in chemistry but to develop a truly dynamical point of view, namely to know the causes, to understand the forces to which man owed the great and manifold successes of modern times.[46] From his other letters, we know that by the "manifold successes of modern times" he meant not only the scientific investigations of forces but also the application of power to industry, war, agriculture and the general wealth of nations.

As we have seen in *Animal chemistry* and *Agricultural chemistry*, Liebig followed Newton (probably with Kant in mind too) in denying man's ability to comprehend the essence of force. In the third edition of *Familiar letters* , which was much longer than the earlier ones, he developed that theme, asserting that the only fruitful quest in dynamics was for the *relations* among forces. He was now prepared to posit, more explicitly than before, an actual interdependance or correlation among forces. Thus, in Letter XIX he suggested that just as naturalists could not define the boundary between plant and animal life, so natural philosophers could not distinguish the boundary between the vital and the physico-chemical forces when they studied the microscopic living creatures.[47] In Letter XX, on 'The connection of the sciences', he again argued the inscrutability of the essence of force, but mentioned some indication of progress in the "wonderful connections" that had been detected between the electrical and chemical forces.[48] In this direction lay the future of science. Letter XXI took up this point:

> "The history of science gives us the consoling assurance that we shall succeed, by pursuing the path of observation and experiment, in unveiling the mysteries of organic life, and that we shall be enabled to obtain decided, definite answers to the question - What are the causes which have a share in producing the vital phenomena? All the peculiarities of bodies, all their properties, are determined by the co-operation of several causes; and it is a problem to be solved by scientific research, to ascertain the proportion in which each individual cause contributes to the effect. In order to attain a knowledge of the mutual relations of these properties, we must endeavour to become acquainted with them, and to discover the cases in which they vary. It is a natural law, which admits of no exception, that variations in one property are always and invariably accompanied by uniform and corresponding variations in another property, and it is perfectly obvious that if we know the laws of these variations, we are enabled to deduce one property from another without further observation.
>
> To ascertain a natural law is nothing more than to ascertain such a relation of dependance. Knowledge of the law includes explanations of the phenomenon, and an insight into the essence of the forces by which it is determined".[49]

All of which was clearly useful, not only to confirm the reality of force, which Liebig never doubted, but also to confirm the existence of vital force.

Liebig went on to enumerate the progress that had been made in discovering the inter-dependencies between forces - between electricity and magnetism, radiant heat and magnetism, radiant heat and electricity. Who could doubt, he asked, that the vital force must also obey the law of dependence, and that the physico-chemical properties of an organism play a definite and ascertainable rôle in vital phenomena?[50]

In this last letter, Liebig seems to have had the studies by Grove, Joule and others in mind for this was precisely the patch they had been cultivating; but as usual he cited no one. Actually, he was well acquainted with their investigations, for we find lengthy discussions of them in some of the volumes of the *Annual report of the progress of chemistry, and the allied sciences, physics, mineralogy and geology* for the 1840s, of which he had been a principal editor. In Volume I (1847)[51] there was a detailed account of experiments by Joule, Dulong and Séguin to determine the mechanical equivalent of heat. This was clearly considered by the editors of the *Annual report* to be an important topic, but it did not seem to them to presage any principle of more fundamental and general import; it was only concerned with two dynamical agents - mechanical work and heat. Séguin was the only one of the three, according to the report, whose thoughts had a wider horizon, for he

> "announces that he is engaged in an extensive series of investigations, in order to determine that the phenomena of heat are only phenomena of motion, and consequently subject to the law of general gravitation".[52]

That *Annual report* carried another discussion of this field at the start of its section on kinetics.[53] It began with a review of Grove's pamphlet 'On the correlations of physical forces' (1846), and what is most interesting is that the editor(s) declared that Grove's

> "... leading notion, that each of the following forces, motion, heat, electricity, light, magnetism and chemical attraction, can be connected into all the others is not altogether new, and perhaps the author has not supported his position with all the materials which were at his disposal ...."[54]

This assessment was grossly unfair: Grove himself did not pretend that his theory was original, but what he did claim, and justly so, was that he had investigated the topic with unprecedented experimental care and detail; yet the review did not bother to praise the thoroughness of his work. Perhaps the reviewer, whoever he was, had in mind the speculations on the interconversions of forces of the *Naturphilosophen*, when he mentioned that Grove's idea was not altogether new; or perhaps he had in mind certain assertions of the same idea which had already appeared in the writings of one of the *Annual report*'s editors, namely Liebig's. (I have not yet discovered who wrote the review. Clearly, it could be most instructive to find that out.) The review went on to approve Grove's remark[55] that the next important task was to determine the mechanical equivalents of the various forces; it mentioned that steps had been taken already by Joule, Karl Friedrich Gauss (1777-1855) and Wilhelm Eduard Weber (1804-1891) on the relations of magnetic forces, and by Weber alone on the electro-chemical equivalent of water. Moreover, the review went on to mention that

> "Matteucci has instituted a series of experiments, which seemed to entitle him to conclude that the imponderables - heat, light and electricity - are developed in chemical processes in quantities independent of each other, so that the amount of the first contains no alteration by the simultaneous appearance of a second or third of them".[56]

The next contributor whom the article discussed was a Mr. Robert Leslie Ellis (1817-59)[57] who had contributed a purely theoretical, wholly mathematical analysis of force to the Cambridge Philosophical Society; he had treated all forces in the form of a single algebraic series which was ingenious but too abstract to be useful or to merit discussion here.

The significance of these *Reports* for our study of Liebig's dynamics is that he obviously knew about the studies on forces of other thoroughly reputable natural philosophers by 1847 at the latest, even though he did not cite them in his own discussions on force in the post 1847 editions of *Familiar letters*. Not even Matteucci's discussions, which had a physiological background, did he cite. We cannot help asking why he refused, throughout his career, to acknowledge other men's work. As in his disputes with Dumas and Boussingault,[58] we know that this parsimony sometimes embroiled him in vicious squabbles. Perhaps he was driven by his well-known ambition to vaunt German science, and especially German chemistry, over that of the French. It is also well-known that he was jealous of his own prestige and consequently became personally involved in his scientific debates; Liebig certainly saw himself as the saviour of physiology and the creator of the rigorous science of organic chemistry. (Indeed, he reminds one of Galileo, whom he admired enormously, for Galileo had had no mean opinion of himself as a physicist and astronomer and did not brook rivals in his own field. We need only recall his treatment of Kepler, and his possessive declaration to Sarsi in *Il Saggiatore* that "it was granted to me alone to discover all the new phenomena in the sky and nothing to anybody else").

Liebig's fierce pride presents us with a problem: If he considered his view on forces to be important and largely original, to the extent of rarely citing any other investigators, why did he never come out with a clear declaration of it? Was it because the two German natural philosophers to enunciate the principles of conservation and correlation in formal papers devoted exclusively to those principles were Mayer and Helmholtz? They were apparently reductionists who seemed to be giving powerful arguments against vitalism, whereas Liebig was still a vitalist. Perhaps he realized the danger to his vitalistic belief in siding with men like them.

Liebig's last major defence of his vitalism was a general critique of contemporary materialism in a lecture given in 1856.[59] His criticism of opponents of vitalism was even more biting than his usual polemics; he called them "total strangers to all investigations connected with chemical and physical forces ... amateurs ... ignorant and presumptuous dreamers".[60] Only insufficient knowledge of inorganic forces had led people to deny an active force in organized beings; a profound understanding of Nature's powers would convince them that the agent responsible for the forms and complex compositions of organized systems had to be different from all other agents yet also strongly akin to them.

The great irony of Liebig's vitalism is that his 19th century biographers mentioned little, or even nothing, about it. Most of them took his own assessment of himself as a sober, thoroughly experimental chemist who had escaped the tentacles of *Naturphilosophie* and other metaphysics at face value. Only his pupil, Theodor Bischoff, gave a reliable assessment of his vitalism.[61] Jacob Volhard mentioned it scantily, as if it was embarrassing.[62] In his otherwise valuable *Faraday lecture* of 1875, August von Hofmann, another pupil, declared that Liebig had not been a vitalist.[63] The biographies by Kohut and Shenstone in 1895 mentioned his vitalism but briefly.[64] Clearly by the end of Liebig's life, vitalism was fast becoming a sign of primitiveness in a scientist's mental outlook. In my opinion, the main flaw in these biographies, even Bischoff's, was not that they ignored Liebig's vitalism, but that they missed his intense interest in force and energy and his contribution to the emergence of the correlation and conservation laws. In vain one searches them for an awareness of the dyanamical aspect of Liebig's physiology, and that is because they focussed on his techniques and discoveries in organic chemistry, in which his vital force had no apparent place.

Some 20th century studies have been redressing the balance, however, and Stephen Toulmin,[65] June Goodfield,[66] T.S. Kuhn, F.L. Holmes and Timothy Lipman[67] merit particular mention. As Lipman asserted, Liebig was able to be a splendid chemist, as well as a full-committed vitalist, simply because he could do his chemistry without employing the vital force. Vital force was relevant solely to his profoundest theorizing in physiology; it was a shadowy figure in a cupboard that could be kept tightly locked when he wished to wear the hat of a thorough chemical experimentalist. Yet, as this chapter has argued, albeit all too briefly, the topic of force, energy and work was a cornerstone of his metaphysic of science, and energy conservation was a principle be believed to apply equally to the realms of life and of brute matter.

NOTES TO CHAPTER 14.

1. T.S. Kuhn, 'Energy conservation as an example of simultaneous discovery', in Marshall Clagett (editor), *Critical problems in the history of science*, Madison, Univ.of Wisconsin Press, 1959, pp.321-356.

2. *Ibid.*, p.334.

3. J. Liebig, *Die Thierchemie oder organische Chemie in ihrer Anwendung auf Physiologie und Pathologie*, Braunschweig, Friedrich Vieweg und Sohn, 1842. I have also read, and shall refer usually to the 1964 reprint of the first English translation by William Gregory, namely *Animal chemistry, or organic chemistry in its application to physiology and pathology*, edited from the author's manuscript by W. Gregory, (1842), with a new introduction by F.L. Holmes, New York and London, Johnson Reprint Corporation, 1964.

4. J. Liebig, *Chemische Briefe*, Heidelberg, C.F. Winter, 1844.

5. J. Liebig, *Die Chemie in ihrer Anwendung auf Agrikultur und Physiologie*, Braunschweig, F. Vieweg, 1840. I have read, and shall refer usually to the first English edition, *Organic chemistry in its applications to agriculture and physiology*, edited from the manuscript of the author by Lyon Playfair, London Taylor and Walton, 1840.

6. Holmes, *op.cit.*, Note 3 above, pp.LXXIII-LXXIV.

7. See O.J.R. Howarth, *The British Association for the Advancement of Science: a retrospect, 1831-1931*, 2nd edition, London, published by the Association, 1931.

8. *Op.cit.*, Note 5 above, p.39.

9. *Ibid.*, p.56.

10. *Ibid.*, p.58.

11. *Ibid.*, p.134.

12. *Ibid.*, pp.343-344.

13. Discussed in J.S. Fruton, *Molecules and life. Historical essays on the interplay of chemistry and biology*, New York, John Wiley, 1972, pp.22-86.

14. *Op.cit.*, Note 5 above, pp.353-354.

15. *Ibid.*, p.354.

16. *Ibid.*, p.355.

17. *Ibid.*, pp.357-358.

18. *Ibid.*, p.383.

19. *Ibid.*, original 1842 German edition, p.206.

20. *Ibid.*, pp.208-209. In the Holmes 1964 English edition, p.194.

21. *Ibid.*, Holmes edition, p.194.

22. *Ibid.*, p.207.

23. *Ibid.*, p.199.

24. *Ibid.*, pp.221-242.

25. T. Lipman, 'Vitalism and reductionism in Liebig's physiological thought', *Isis*, 1967, 58:167-185.

26. *Op.cit.*, Note 3 above, pp.211-212.

27. *Ibid.*, pp.28-33.

28. *Ibid.*, p.27.

29. *Ibid.*, p.28.

30. *Ibid.*, p.30.

31. *Ibid.*, p.31.

32. *Op.cit.*, Note 1 above, p.322.

33. *Op.cit.*, Note 3 above, p.31.

34. Lipman, *op.cit.*, Note 25 above, p.176.

35. *Op.cit.*, Note 3 above, p.185.

36. Ernst von Meyer, *A history of chemistry*, translated by G. McGowan, London, Macmillan, 1898, pp.262-271.

37. J.T. Merz, *A history of European thought in the nineteenth century*, New York, Dover, 1965, pp.215-225.

38. Quoted in von Meyer, *op.cit.*, Note 24 above, p.264.

39. There has been one published study on the relationship between von Platen and Liebig, namely by Moritz Carrière, '*Liebig und Platen*', in the *Münchener Allgemeine Zeitung* for 1873. However, I have been unable to get hold of this and have relied upon the two following biographies for my information: (i) Adolph Kohut, *Justus von Liebig, sein Leben und Wirken*, Giessen, Emil Roth, 1904, which has a useful bibliography; and (ii) W.A. Shenstone, *Justus von Liebig, his life and work*, London, Cassell, 1895.

40. *Op.cit.*, Note 3 above, p.7.

41. *Ibid.*, p.186.

42. *Ibid.*, pp.8-9.

43. *Ibid.*, p.9.

44. *Op.cit.*, Note 1 above.

45. J. Liebig, *Familiar letters on chemistry*, trans. by J. Gardner, London, Taylor and Walton, 1844, p.112.

46. *Ibid.*, especially letter 1, pp.1-34.

47. J. Liebig, *Familiar letters on chemistry*, 3rd edition, trans. by W. Gregory, London, Taylor and Walton, 1851, p.247.

48. *Ibid.*, pp.257-259.

49. *Ibid.*, pp.264-265.

50. *Ibid.*, pp.271-272.

51. J. Liebig and H. Kopp, *Annual report of the progress of chemistry and the allied sciences*, edited by A.W. Hofmann and W. de la Rue, London, Taylor and Walton, 1849, pp.41-44.

52. *Ibid.*, p.42.

53. *Ibid.*, pp.93-94.

54. *Ibid.*, p.93.

55. *Ibid.*, p.93.

56. *Op.cit.*, Note 49 above, p.94.

57. Cited in *ibid.*, p.94. Ellis's paper, which I have not read, is cited as: *Phil.Mag.* /3/ 33:393.

58. See von Meyer, *op.cit.*, Note 34 above, pp.274-281. Also Shenstone, *op.cit.*, Note 37 above, pp.51-61. And Holmes, *op.cit.*, Note 3 above, pp.xxx-xxxi.

59. Cited in Lipman, *op.cit.*, Note 25 above, pp.182-183. The lecture eventually appeared as letter 23 in *Chemische Briefe*.

60. Cited in *ibid.*, p.183.

61. Theodor L.W. von Bischoff, *Ueber den Einfluss des Freiherrn Justus von Liebig auf die Entwicklung der Physiologie*, München, K.B. Akademie, 1874, pp.76 and ff.

62. J. Volhard, *Justus von Liebig*, Leibzig, Barth, 1909.

63. A.W. Hofmann, *The Faraday lecture, 1875: The life-work of Liebig in experimental and philosophic chemistry*, London, Macmillan & Co., 1876.

64. Kohut, *op.cit.*, Note 39 above. And Shenstone, *op.cit.*, Note 37 above, pp.167-169.

65. Liebig's vital force is compared with other theories of vitality, of the 18th and 19th centuries, in Stephen Toulmin and June Goodfield, *The architecture of matter*, London, Hutchinson, 1962, pp.324-330.

66. G.J. Goodfield, *The growth of scientific physiology*, London, Hutchinson, 1960, pp.135-149.

67. In addition to *op.cit.*, Note 25 above, see T.O. Lipman, 'The response to Liebig's vitalism', *Bull.Hist.Med.*, 1966, 40 :511-524.

CHAPTER 15. Ideas on force of Karl Reichenbach (1788-1869)

"*Einer neuen Wahrheit ist nichts schädlicher als ein alter Irrthum.*" Goethe.

One of the most enigmatic investigators into force during the 1840s and 1850s was Karl von Reichenbach. Having studied at Tübingen, where he obtained the degree of doctor of philosophy, he set out to apply his scientific knowledge to industry and established several metallurgy factories in Moravia. His efforts were successful, for he acquired a large fortune as well as a baronetcy in 1830 from the King of Würtemberg. In purely scientific circles he was known as the author of the first geological monograph to be published in Austria,[1] he discovered paraffin[2] and creosote[3] in 1830 and 1833 respectively, and he was an authority on meteors.[4] He attracted most attention, however, although his scientific reputation was proportionately lessened, by his supposed discovery of a new force in Nature, which he called the *Od*.

The task for an historian of science of evaluating the rigour and worth of von Reichenbach's theory of the *Od* is daunting, for since his earliest announcement of it he has been the object of ridicule on the one hand (by Emil du Bois-Reymond particularly), and considerable praise on the other (by Berzelius, William Gregory and William Carpenter, to name only three). Indeed, any man who would allow his researches into light, heat, electricity and magnetism to lead him into that *noli me tangere* of mid 19th century science - animal magnetism - was asking for trouble. Not that von Reichenbach was a follower of Mesmer.[5] Indeed, he declared often that

Mesmer's "hotch-potch of the most absurd kind" was neither science nor anything like his own sober theories. His experimental researches - and they were truly experimental, not merely fortuitous fact finding - especially those on the human body, led him by quite straight-forward induction to postulate the *Od*, which William Gregory, his English editor and champion (who had also edited and championed Liebig's work) renamed the *Odyle* or *Odyllic Force*.

Von Reichenbach published his first detailed account of his dynamical researches as a 270 page supplement[6] to Liebig's and Wöhler's *Annalen der Chemie und Pharmacie* of 1845. In Germany it was received with scepticism and even ridicule. Emil du Bois-Reymond wrote a biting review of it for Karsten's *Fortschritt der Physiologie* of that year,[7] calling it "an absurd romance, to enter into the details of which would be fruitless", and "one of the most deplorable aberrations that has for a long time affected a human brain".[8] This criticism was obviously not based on a careful, objective reading of von Reichenbach's work; it was almost certainly based on his impression that this was yet another piece of hocus-pocus in support of the then thoroughly suspect field of Mesmerism or animal magnetism; and as Gregory suggested, he probably did not even bother to read the whole of it.

Von Reichenbach, to his eternal credit, did not stoop anywhere as low as his critics. Whilst their criticisms were often venomous, his replies were calm and polite. There were several likely reasons for his equanimity. Firstly, he saw his work firmly within the recent, careful studies on dynamics that had been done by eminent figures like Faraday, Mrs. Somerville and Liebig, all of whom he cited, especially Liebig. Secondly, he appreciated the dangerous ground on which he trod and avoided speculating about the fundamental nature of force. Although he asked occasional questions about the possibility of all forces being correlations of a single one, etc., questions which anticipated the doctrines of the correlation and conservation of forces, they were always only tentative and he never gave definite answers to them himself. In short, he realized how difficult it was to treat *Kraft* theoretically. Thirdly, his work on force was highly regarded by several eminent scientists, whose statures were an easy match against du Bois-Reymond. However bad-mannered his critics might be, he was not alone.

In 1846 William Gregory, then professor of chemistry at Edinburgh, disappointed at von Reichenbach's treatment in Germany, translated his *Untersuchungen* under the title of *Abstract of 'Researches on magnetism and on certain allied subjects', including a supposed new imponderable'*.[9] Although only an abstract, it was 112 pages long and was crammed with accounts of experiments, usually by von Reichenbach himself. They described how lights or flames had been seen around powerful magnets, usually at their poles; how magnets and crystals had exerted forces on parts of the human body; how light had been emitted during crystallization of common salts; and how the thitherto unnamed force resembled, yet also differed from, the other forces of the universe. The experiments had usually been on people who were highly sensitive, often nervous and sometimes suffering from or recuperating from serious nervous disorders. This was a point on which du Bois-Reymond picked, but von Reichenbach himself had admitted this weakness:

> "From what has been said, it appears that the peculiar force thus detected opens up a new leaf in the book of the imponderables. The new force appears to be subject to the general laws of the imponderables, but has its specialities and peculiar laws, the study of which must henceforth be a problem of physics. It is exceedingly desirable to discover an inorganic test or reagent for it, a means of recognizing and measuring it, which shall relieve us from the dependence ... on sick persons, hospitals, and unscientific persons of all kinds. The author is engaged in this research and has good hopes of success".[10]

The problem was that because this field had never been amenable to rigorous, quantitative experimentation with the tools of physics and chemistry, scientists were neglecting it. Physiologists like du Bois-Reymond (and Ludwig, Helmholtz and Brücke) had chosen *a priori* to exclude from their science all phenomena that could not be manipulated by physics and chemistry. Their definition of valid science was different from, and at least as questionable as, von Reichenbach's. As he put it:

> "... scientific men neglected the subject, and did not admit it as a branch of physical enquiry. Individual physicians and lay amateurs partly kept alive the tradition ... they called it Animal Magnetism .... Numerous works have since that time appeared on the subject, chiefly written in a medical point of view. A few are good; many partial and one-sided; many, again, such as cannot be read with patience.

> The author has avoided the study of this literature, in order to obtain an unfettered judgement, and to raise his work on the foundation of his own observations. He has studied the subject in a physical, not a medical point of view, being convinced that thus the investigations will be more successful".[11]

Actually, since so many of his experiments were done on the human body, his ideas did have a physiological bearing. That he discussed in a later work.

Numerous experiments done over several years had convinced him of the existence of a new force. The most exact and unequivocal experiments had suggested that it existed in magnets, crystals, the sun's rays (an area which Mrs. Somerville had been investigating),[12] the human body, sources of heat and the earth's magnetic field. It was also produced along with, or by, chemical force; and thus, without fanciful speculation, he could account for Mesmer's magnetic *baquet* - that it was merely a slow and long-lasting source of chemical activity, and that from such activity came the new force which resided too in the human beings connected up to the *baquet*. Since the motive force in the *baquet* might thus be shown to be due to ordinary chemical activity, its mystique and the ridicule of animal magnetism would dissolve away.[13]

What, then, was the origin of the living organism's magnet-like force? None other than the chemical activities involved in digestion. And his great authority on the relations between organic and chemical forces was Liebig:

> "By one of the most profound combinations of thought to which our age can point, Liebig has led us to the idea that all the motive force in our bodies is produced by digestion, and all the heat by respiration: that is, that both - force and heat - were the result of chemical action. Although this cannot yet be brought into an algebraic formula or a chemical equation, and disputes may occur here and there about the expression of this truly great idea, yet the idea addresses itself so powerfully to our comprehension, and finds so powerful an echo in the general knowledge we possess of Nature, that its ultimate triumph is secure.
>
> The author considers it as no small security for the truth to Nature of his researches, that he has been brought, by a different route, to the same new field of inquiry as Liebig had already opened up".[14]

This was the earliest, full recognition of the dynamical import of Liebig's chemical theory of vitality, of which I am aware. Much of the rest of the treatise was taken up with applying this idea to particular vital processes, and the message was always clear: the living organism cannot manufacture its own power, and even its most vitalistic forces, even the *Od*, are only metamorphoses of the chemical power that it ingests in its food and inspires as oxygen. Like Liebig and others discussed in this thesis, von Reichenbach would not be drawn on the ultimate nature of the forces; perhaps they are essentially one;[15] perhaps the *Od* is merely a hitherto unrecognized modification of a well-known force;[16] even with regard to the positive-negative polarity that the *Od* seemed to possess, he was uncertain;[17] he hoped merely, in future researches, to conduct comparative experiments on forces and to illustrate whatever *relations* might exist among them. It would be, he modestly declared, for higher authorities to pass judgements on the field as a whole.[18]

His editor, Gregory, appreciated the difficulties that would waylay the theory of *Od*. It was only just on the fringe of respectable science yet, as he pointed out in his preface, there were a few very respectable scientists working on that fringe. Faraday was cited as having corroborated von Reichenbach's observation of the luminous phenomena associated with the magnet,[19] and a comparison was made with the work of John W. Draper in New York who, from his study of the sun's rays, had suggested the existence of a new imponderable.[20] In Gregory's opinion, "the current of discovery seems to set in that direction".[21]

A larger English publication of von Reichenbach's work appeared in 1850, entitled *Physico-physiological researches on the dynamides or imponderables, magnetism, electricity, heat, light, crystallization and chemical attraction, in their relations to the vital force*.[22] The editor was again Gregory. Part I was an improved edition of the 1846 *Abstract*. Part II and the appendices described the further investigations that von Reichenbach had promised to do since 1845; these latter are immensely important for this thesis.

The first section in the book which merits comment is Gregory's preface. He claimed that the *Abstract* had been well received in England and that he had not met any serious criticisms by British philosophers. Of Part II he wrote that von Reichenbach had done many comparative experiments on *odyle* and the other imponderables and had established their analogies; he had also shown their differences,

> "which leave no choice for the present, but that of giving it a distinct place and name, although future discoveries may possibly enable us to refer *all* the imponderables to a common force. But that time is still distant".[23]

As for the German critics, Gregory claimed that they simply did not understand the proper objects of scientific inquiry or the nature of admissible evidence when investigating obscure and difficult problems. Had du Bois-Reymond been more enlightened on these points, he would have realized that von Reichenbach's researches actually supported his own ideas.[24]

In his own preface, von Reichenbach discussed du Bois-Reymond, suggesting that he had taken his critical cue from his teacher, Müller, who had inveighed against Mesmerism or animal magnetism in his *Handbuch*. Von Reichenbach quoted Müller's works.[25] However, Müller had not had him in mind because the *Handbuch* was written before his work became known. Moreover, von Reichenbach (and Gregory) agreed with Müller's criticism of Mesmerism. Thitherto, animal magnetism had been a charade, but now von Reichenbach believed that a truly rigorous approach had been found to those pehnomena which had gone under the umbrella of Mesmerism. Indeed, so scientific and new was his approach that he could not regard himself as a champion of animal magnetism at all; it differed from his theory of *Od* as alchemy differed from chemistry. (In this respect, therefore, one cannot agree with Garrison's assertion that

> "mesmerism ... was exploited in various mystic forms by /*inter alia*/ ... Baron Karl von Reichenbach whose concept of the odic force still survives in the ouija-boards and odic telephones of the present time").[26]

The kernel of von Reichenbach's theory, as set out in his *Physico-physiological researches* was far from mystic: just as magnets, crystals, living organisms (those with nervous systems), solar and lunar rays, heat and electricity possess the power of exerting particular forces, so does the Od reside in many diverse bodies and takes its place as a universally diffused natural force. In his own mind, it was a plain consequence of induction from his many experiments. As we might expect, however, he did admit the appeal of a more grandiose hypothesis which smacked strongly of *Naturphilosophie*; but he never cited the *Naturphilosophen* or Kant, and mentioned an all-encompassing hypothesis on forces only tentatively, preferring to keep his ideas close to their empirical foundations. Thus he discussed the similarities between his work and Faraday's on the newly named phenomenon of 'Diamagnetism', suggesting that they were merely "drawing the same vehicle, but by different ropes." Faraday had grasped one of "the numerous odyllic threads", and with his genius would help discover the ultimate basis of such phenomena. Perhaps magnetism, diamagnetism and odyle would be reduced to a common origin - but the proof was still far distant.[27]

Still, the lure of a grand vision of Nature's forces and the intuitive feeling that some monumental unifying law lay waiting to be discovered were too much for von Reichenbach's customary restraint. His Od would one day be seen in true cosmic perspective:

> "It has always been viewed as more or less identical with magnetism; but we have seen that it has no greater resemblance to that force than magnetism has to crystallization, crystallization to electricity, electricity to chemical attraction, heat to light etc. We no doubt have a presentiment of the final unity of these imponderables in a higher form; but we are still far removed from this much-desired goal of natural science. We cannot yet fill up the gap between magnetism and electricity, which appears so narrow that we might almost expect to reach with our hands from one bank to the other".[28]

Summary

From such writings by von Reichenbach there is no doubt that he was a capable experimentalist and appreciated the need for experimental proof. Nor can we doubt that he was aware of some great unifying principle of force. Neither can we deny the care with which he refrained from generalizing and speculating in a field which was rampant already with generalizations and exciting speculations. Born and bred in the age of *Naturphilosophie* he somehow remained aloof from its flighty mentality, and though well acquainted with its speculations - such as the unity of forces - he kept his own ideas fairly closely to the ground of empiricism. It is inaccurate to ask, as one commentator has done, "... how a man who had shown sharp critical understanding in science could wander so far in the field of fantasy".[29] He undoubtedly saw himself among those thorough researchers who were investigating force, and in a sense he was truly one of them; Liebig was his mentor in physiological dynamics (although Liebig did not think much of his work and, to my knowledge, never cited him), and Faraday was his model in the physical sciences. Yet in another sense he was apart from them: he never discussed Helmholtz or Mayer and mentioned only a few of the British workers; Grove and Joule were not cited in any of his pre-1852 writings.

In his later years, von Reichenbach stuck resolutely to experimenting and writing on his Od. In 1852 he published *Odisch-Magnetische Briefe*,[30] in 1854 *Der sensitive Mensch und sein Verhalten zum Ode*,[31] in 1856 *Odische Erweiterungen*[32] and *Köhierglaube und Afterwissenschaft*,[33] in 1866 his peculiar *Aphorismen über Sensibilität und Od*[34] and in 1867 *Die Odische Hohe*.[35] From a scanty perusal of these treatises one gets the impression that, piqued at the world's refusal to take him seriously, (Gregory died in 1858 and no-one would take up his cudgels in Britain), he retired to his castle of Reichenberg and worked away in splendid isolation, even to the extent of ignoring the steady stream of works that appeared in the 1850s and 1860s concerning the correlation and conservation of forces. In 1862 he gave a demonstration of his supposed ability to photograph objects in total darkness by means of their 'odic light' to a group of seven academic scientists in Berlin;[36] he himself thought that his demonstration succeeded, but they issued a press statement denying it. According to one of his biographers,[37] that sounded the death-knell for his scientific credibility.

To the end of his life he denied that a general unifying principle of force, so much desired even by him, had been found. The Od was his sole *raison d'être*, and in a last-ditch attempt to gain its acceptance he visited the eminent physicist, experimental psychologist and philosopher, Gustav Fechner (1801-1887). A few months later, von Reichenbach died and Fechner wrote an account[38] of his visit. Though not convinced, Fechner was not unsympathetic and frankly admitted that he could not detect fraudulence in von Reichenbach's quite impressive demonstrations.

What can an historian make of von Reichenbach's work on force? Was it a long day-dream? Or was it a series of often thorough, though sometimes bizarre, experiments leading him into a fringe area of science which, through the ill-repute gained for it by Mesmer, would never be accepted as a legitimate topic for science, no matter how plausible the evidence? As Goethe's saying at the head of this chapter goes, "Nothing is more hurtful to a new truth than an old error." My own evaluation is that his work had some of the makings of a thorough and important research programme and until the early 1850s was close to the main line of ideas on force and energy. Thereafter, he began to build the Od into an obsession and neglected the more general aspects of his work. Perhaps if he had held an academic post in a German university, with professional investigators around him to discuss his ideas, he might have left the Od alone. If he had had Helmholtz's mathematical expertise, or Mayer's delight for metaphysics, he might have done more on his belief in the fundamental correlation of forces and might have earned a reputable place in the history of 19th century studies on force. As it was, he was surely one of those natural philosophers who reflected, and in some measure contributed to and appreciated the importance of, those studies. Perhaps some words by Helmholtz describe his situation:

> "Conscientious workers who are shy at bringing their thoughts before the public before they have tested them in all directions, solved all doubts, and have firmly established the proof, are at a decided disadvantage".[39]

NOTES TO CHAPTER 15.

1. K. von Reichenbach, *Geologische Mittheilung aus Märken*, Vienna, W. Braumüller, 1834.

2. Between 1830 and 1833 von Reichenbach wrote 18 papers under the title of 'Contributions to the knowledge of dry distillation of organic substances' for Schweigger's *Journal für Chemie und Physik*. In the first of these papers he announced the discovery of a substance from beech-wood tar, which he named 'paraffin' from the Latin *parum*, 'little', and *affinitas*, 'affinity'. The name expressed paraffin's unreactivity with even the strongest acids and alkalies, and he recognized at once its potential usefulness: "It promises to give table candles a satisfactory new material. It can also give better service than any previously known substance in covering materials and vessels which must be rendered acid-resistant".

   It was this work which first drew Berzelius's attention to him. Despite his penchant for business ventures, however, he never made paraffin on an industrial scale. That was first done by James Young (1811-1883) in England in 1850, after he had found that Scottish coal gave better yields than beech.

3. In 1833 he reported his extraction of an alkali-soluble substance from beech-wood tar, calling it creosote and characterizing it as follows: "A small amount dissolved in water kills small animals. It is the mummifying principle of pyroxylic acid. Flesh dipped in the water solution no longer spoils. There is no doubt that it

will find medicinal uses ".

4. His work on meteorites, which began after his work on beech-wood tar, resulted in 28 contributions, some very lengthy, to Poggendorf's *Annalen der Physik und Chemie* between 1850 and 1865.

5. It is not an intention of this thesis to discuss the theory of Mesmerism, although it would not be totally irrelevant. However, a brief synopsis of the ideas of Friederich Anton Mesmer (1734-1815) might not be out of place here. Mesmer, a native of Switzerland, had graduated in 1771 with a thesis on the influence of the planets on man, and in experiments with the magnet he concluded that a similar power is possessed by the human hand. He practised mesmeric therapy in Vienna, was expelled, and in 1778 established a successful practice in Paris. A principal feature of mesmeric treatment was a so-called magnetic *baquet* or tub, containing a mixture of all kinds of stuff, from which silk or iron threads emanated which his patients held in their hands. There was a large amount of hypnosis and theatricality in these séances and inevitably his practice was investigated by an official commission. It was roundly condemned, and after the revolution Mesmer dropped out of sight. He described his theory in his *Mémoire sur la découverte du magnétisme animal*, Geneva and Paris, P.F. Didot le jeune, 1779. This synopsis is taken from F.H. Garrison, *An introduction to the history of medicine*, 3rd edition, Philadelphia and London, W.B. Saunders, 1924, p.382.

6. K. von Reichenbach, *Untersuchung über den Magnetismus und damit verwandte Gegenstände*, *Annalen der Chemie*, 1845, 53, separately paginated pp.1-270.

7. Cited in *op.cit.*, Note 22 below, p.xxxiv.

8. *Ibid.*, p.xxxiv.

9. K. von Reichenbach, *Abstract of 'Researches on magnetism and on certain allied subjects', including a new supposed imponderable*, edited by W. Gregory, London, Taylor & Walton, 1846.

10. *Ibid.*, pp.25-26

11. *Ibid.*, p.28.

12. *Ibid.*, p.52.

13. *Ibid.*, p.65.

14. *Ibid.*, pp.66-67.

15. *Ibid.*, p.66.

16. *Ibid.*, p.89.

17. *Ibid.*, p.91.

18. *Ibid.*, p.89.

19. *Ibid.*, p.vi.

20. *Ibid.*, pp.vii-ix.

21. *Ibid.*, p.ix.

22. K. von Reichenbach, *Physico-physiological researches on the dymanides or imponderables, magnetism, electricity, heat, light, crystallization and chemical attraction, in their relations to the vital force*, edited by W. Gregory, London and Edinburgh, Taylor, Walton & Maberley, 1850.

23. *Ibid.*, p.xi.

24. *Ibid.*, p.xxiii.

25. *Ibid.*, p.xxxviii.

26. Garrison, *op.cit.*, Note 5 above, p.382.

27. *Op.cit.*, Note 22 above, pp.228-229.

28. *Ibid.*, p.164.

29. Moritz Kohn, 'Karl Freiherr von Reichenbach (1788-1869). Investigation of paraffin (1830) and wood tar (1833)', *Jour.Chem.Ed.*, 1955, 32:188-189.

30. K. von Reichenbach, *Odisch-Magnetishe Briefe*, Stuttgart u. Tübingen, J. G. Cotta, 1852.

31. K. von Reichenbach, *Der sensitive Mensch und sein Verhalten zum Ode*, 2 vols., Stuttgart, J.G. Cotta, 1854-1855.

32. K. von Reichenbach, *Odische Erweiterungen*, Vienna, W. Braumüller, 1856.

33. K. von Reichenbach, *Köhierglaube und Afterwissenschaft*, Vienna, W. Braumüller, 1856.

34. K. von Reichenbach, *Aphorismen über Sensibilität und Od*, Vienna, W. Braumüller, 1866.

35. K. von Reichenbach, *Die Odische Lohe*, Vienna, W. Braumüller, 1867.

36. According to W.V. Farrar in the *Dictionary of scientific biography*, New York, Charles Scribner, 1975, 11:359-360, it was an impressive group, comprising Christian Gottfried Ehrenberg (1795-1876), Heinrich Gustav Magnus (1802-1870), Heinrich Rose (1795-1864), Eilhard Mitscherlich (1794-1863),

Johann Christian Poggendorff (1796-1877), Peter Theophil Riess (1804-1883) and K.H. Schellbach.

37. Kohn, *op.cit.*, Note 29 above, p.188.

38. G.T. Fechner, *Erinnerungen der letzten Tage der Odlehre und ihres Urhebers*, Liepzig, Breitkopf und Härtel, 1876.

39. Helmholtz, *op.cit.*, Note 2, chapter 16 below, pp.228-229.

CHAPTER 16. On the principle of the conservation of power as enunciated by Hermann Helmholtz (1821-1894) from his work in physiology

> "The first discovery of a new law is the discovery of a similarity which has hitherto been concealed in the course of natural processes. It is a manifestation of that which our forefathers in a serious sense described as 'wit'; it is of the same quality as the highest performances of artistic perception in the discovery of new types of expression. It is something which cannot be forced, and which cannot be acquired by any known method."
> [Hermann von Helmholtz, in 'On thought in medicine'.]

The aim of this chapter is humble. It does not try to describe in great detail the work of Hermann Helmholtz on the concept of force, for a sufficient number of monographs have been written already on his life's work. In any case, I am far from confident in my own understanding of his ideas in their entirety. My aim is to describe the physiological investigations out of which his early ideas on force seemed to arise and to discuss, albeit too briefly, the metaphysical, *a priori* reasons he had for believing in the theory of conservation. My task will not be original in that other scholars have trod the path already.[1] However, my version has relied greatly on my own reading of the primary sources, even those which were written long after the 1840s and which might seem at first sight utterly irrelevant to the topic in hand, and occasionally I have seen fit to mention a fact or suggest an interpretation that other scholars (so far as I recall) have not.

The signs of Helmholtz's polymathic genius appeared early in his life. Encouraged by his father who taught philosophy and philology and was a friend of the younger Fichte, he developed an early interest in philosophy, especially in Kant and the elder Fichte, which despite his enormous achievements in experimental science, he retained throughout his life. Although he wanted to read physics at university, he was sent to the Königlich Medizinisch-chirurgische Friedrich-Wilhelms Institut in Berlin to read medicine. Yet he did not regret his medical training, for it provided him with a useful pattern of thought, a virgin field for scientific discovery and some acquaintance with physics and chemistry.[2]

In Berlin his three greatest teachers were Johannes Müller in physiology, Eilhard Mitscherlich (1794-1863) in chemistry and zoö-chemistry, and Heinrich Gustav Magnus (1802-1870) in physics. Müller, with whom he had most contact, and Magnus were strong proponents of the movement, then arising in German universities, which rebelled against the excessive importation of metaphysics and sought to investigate living phenomena with physico-chemical tools.[3] Under their influence, an alliance was forged between physicists and chemists on the one hand and certain physiologists on the other, and within this spirit of cooperation a group of young men formed the *Physikalische Gesellschaft* in Berlin for the discussion of natural phenomena. Among the members of this group were Helmholtz, du Bois-Reymond, Brücke, Ludwig and other physiologists who were to contribute greatly to experimental physiology.[4]

It is important to realize that whilst this young group drew much inspiration from Müller and Magnus, they also departed significantly from their scientific precepts. Whereas Müller always believed in the inscrutability, the unique vitality, of the organism, these brilliant students chose to treat it as a purely mechanistic system. And Magnus, at least for many years, regarded the paramount object in physics to be the collection of raw data, to the almost total exclusion of theorizing. He even regarded experimental and mathematical physics as separate subjects.[5] Soon his pupils abandoned his Baconian approach and showed a readiness to develop hypotheses that Magnus (rightly) considered to be beyond the power of their data.

From Magnus, Helmholtz obtained some guidance in mathematics and physics. From Müller, he seems to have obtained five things worth mention here: 1. The readiness to employ as many techniques as possible - chemical, physical, anatomical or philosophical - in tackling physiological problems. 2. An awareness that scientific theories are only heuristic guidelines which must be sacrificed to empirical data; and that one's most committed theories should continually be defined more precisely. On this latter point he cited Müller's own continual efforts to refine his ideas on the activities of the *Lebenskraft* and the conscious soul.[6] 3. A deep understanding of German philosophy and a belief that the philosophies particularly of Kant and Spinoza were of the utmost importance for science. 4. An interest in, and an awareness of the enormous complexity of, the sense-perceptions. Müller's doctrine of the specific energies of nerves was, in his opinion, "a scientific achievement whose value I am inclined to consider as equal to that of the discovery of the law of gravitation".[7] 5. An interest in the dynamics of the living organism: whether it has its own, inscrutable source of force, or whether there is a strict relation between the work and heat it can produce on the one hand, and the forms of force - predominantly chemical - that it can take in on the other. In brief, the debate between vitalism and mechanicism. It was also a question - as Helmholtz recalled in a lecture many years later - of the possibility of perpetual motion in living systems.[8]

This last issue - vitalism versus mechanicism - was the first research project that Müller set Helmholtz after he had completed his doctoral dissertation in 1842. The particular form it took was the debate over fermentation which, due largely to Liebig's recent work, was a topic of considerable interest. In the late 18th century Lavoisier had recognized that the two products of alcoholic fermentation, namely alcohol and carbonic acid gas, derive solely from the sugar substrate and he attempted to determine the quantitative relation among them. His explanation of the process was a purely chemical one. So too was that of Berzelius in the 1830s, who, having recently enunciated his theory of catalysis,[9] asserted that the yeast or ferment is a catlyst - that it causes the sugar's decomposition by mere contact with it, much as platinum black had been shown by John Davy and Johann Döbereiner (1780-1849) to effect a transformation of methane or alcohol into other substances. For several years this account was quite widely accepted although, as Liebig pointed out, it was no explanation at all, inasmuch as the nature of catalytic action itself was a total mystery.[10]

In 1836, three researchers, Charles Cagniard de Latour (1777-1859) a biologist and physicist in Paris, Müller's pupil Theodore Schwann (1810-1882) in Berlin, and Friedrich Kützing (1807-1893), a botanist and teacher in natural science in Nordhausen in Germany, discovered simultaneously and independently that yeast comprises minute living organisms, whose life and self-propagation seemed to be intimately connected with the chemical phenomena of alcoholic fermentation. Schwann also performed experiments to follow up a study by Gay-Lussac who had shown that well-cleaned grapes or boiled grape juice put into a Torricellian vacuum would not ferment, but if a bubble of air were admitted then they would. Gay-Lussac concluded that the oxygen induced the fermentation, which was therefore to be regarded as a purely chemical process. In 1838 Schwann repeated Gay-Lussac's experiments and showed, furthermore, that if the bubble of air were admitted via a red-hot tube, fermentation would not occur. He even showed that 20-24^{o}c. was the most favourable temperature range for the air to permit fermentation - in short, that the conditions favouring fermentation are those one usually associates with living organisms.[11]

In 1839 Liebig published his first counter-blast defending the chemical view.[12] He did not deny the living nature of yeast, but he did deny that the vital activities of yeast itself effected fermentation. He proposed instead that an albuminous substance (a ferment) exists within living yeast, and that when the yeast dies this ferment is released; that the molecular vibrations in the ferment, which are initiated by contact with oxygen (whose particles, being gaseous, are in continuous motion), communicate themselves to the sugar particles which disintegrate into the smaller particles of alcohol and water. Therefore, with regard to fermentation the life of the yeast was merely an epiphenomenon and not a cause.

This was where Helmholtz came in. Armed with Mitscherlich's textbook on organic chemistry and his treatise on fermentation, he repeated Gay-Lussac's experiments, devised some of his own and published a paper 'On the nature of fermentation and putrefaction'[13] in Müller's *Archiv* in 1843. This paper had set out to support Liebig in arguing against spontaneous generation, (a topic which was closely allied to ideas on fermentation), but it disagreed with him on the nature of fermentation and of putrefaction. (In Liebig's opinion, putrefaction, the dis-intergration of dead organic matter, was in the same category as fermentation, both being due to chemico-mechanical causes). Helmholtz asserted that putrefaction could proceed independently of life - as Liebig had; that it offers fertile ground for the development of living germs and is itself modifiable by them - which Liebig had not asserted; and that fermentation is one such putrefactive process modified by living germs and intimately associated with them - in direct opposition to Liebig. His summary was that putrefaction is essentially non-vital, but that fermentation resembles vital processes in the substrates it uses and in its rates of increase and decrease according to the favourableness of its environment. These results seemed to give fresh support to vitalism; consequently, they were viewed suspiciously by several of his associates, for instance by Magnus, who generously invited him to use his own laboratory and so employ "methods of investigation that would throw more light on the subject than such as a young army-surgeon living on his pay could provide for himself".[14] However, it was not until two years later that Helmholtz could convince Magnus of the accuracy of his work, for he had to begin army service in 1843. Besides, when he resumed his physiological researches he took up a different problem - one on which Liebig had also worked.

In 1845 Helmholtz set out to test Liebig's assertion of a correlation between the mechanical force and the heat that an organism can generate on the one hand, and the chemical forces that are entailed in such generative processes. The issue at stake in Helmholtz's mind, as he admitted in a lecture in 1891, was whether the living organism obeyed the same laws of force as did purely mechanical, inorganic systems, and in particular whether the former system obeyed the principle of the conservation of *vis viva* or allowed perpetual motion. That these issues were clearly in his mind we may be sure, for during his Berlin student days he had read Des Cartes, Newton, Leibniz, Euler, Daniel Bernouilli, D'Alembert and other mathematicians and had become particularly acquainted with Bernouilli's extensive application of Leibniz's concept of *vis viva*.[15] So far as his ideas on force or *vis viva* in physiology were concerned, he had adhered to the commonly adopted explanation of vitality, namely Stahl's,whereby physico-chemical forces within the organism were supposed to cause its physico-chemical activities, but these were regulated by an indwelling life-soul, vital force, or *anima*. After death, the free action of the physico-chemical forces was supposed to cause decomposion.[16] Helmholtz felt that there was something contrary to Nature in this explanation but it took considerable effort to state his misgivings in a precise form. In 1841 he realized the nature of those misgivings: Stahl's theory implied that every living organism could be a *perpetuum mobile*. Helmholtz had heard perpetual motion discussed by his father (as a purely philosophical issue, we might guess) and by his school mathematics teachers; he therefore investigated what Bernouilli *et al* had thought and began to formulate questions like: "What relations must exist among the various natural forces for perpetual motion to be possible? And do these relations actually exist?"[17] However, as he realized at that time, such questions and the answers that the illustrious mathematicians had given up to that time had applied only to inorganic mechanisms - indeed, only to mechanics and not to the chemical, electrical, magnetic and other forces. (This was, as Emile Meyerson points out, because everyone believed that all forces were fundamentally mechanical, and the need to treat chemical and other forces as separate extra-mechanical testing-grounds for *vis viva* had not been envisaged.[18])

As a consequence of studies done on specialized forms of force since the beginning of the century - on chemical forms by Liebig for instance, on animal electricity by a host of workers, and on electromagnetism by Ampère and others - Helmholtz saw his problem between the time of his dissatisfaction with Stahl and his 1847 paper as not only to analyse the relations of force in mechanics, but also to extend that analysis to all other forms of force.[19] Moreover, since he believed by 1845 at the latest that heat was a force or a phenomenon of motion, (he did not try to be more specific), it was clear that in studying its relation to the conservation of *vis viva* and perpetual motion he would be helping to close the gap between vitalism and mechanicism, since animal heat should be as amenable to physico-chemical analysis as ordinary heat. Such was the importance, in his own mind, of the investigations into muscle action, animal heat and nerve transmission that he undertook between 1845 and c.1850.[20]

The above interpretation brings out the full import of a passage that F.L. Holmes, in his admirable introduction to the 1964 reprint of Liebig's *Animal chemistry*, quotes from one of Helmholtz's 1845 papers:

> "One of the highest questions of physiology concerning the existence of the vital force itself, namely whether organic life is the effect of a self-propagating, purposeful force, or the result of forces operating also in inanimate nature but modified by the particular manner in which they are combined, has recently in Liebig's effort to derive physiological phenomena from known chemical and physical laws been given an especially clear and concrete form; namely, whether or not the mechanical force and heat created in the organism can be derived entirely from chemical transformations".[21]

If Liebig was correct, the muscle's contractions should be caused by a detectable change in its composition. Helmholtz took two similar frog-leg-muscles, one of which he stimulated to exhaustion by an electric current whilst the other was left alone. Chemical analysis of the muscles showed that the ratio of alcohol-soluble to water-soluble components was greater in the exhausted than in the quiescent one, and that chemical transformation had occurred. For a long time this experiment was cited as the best evidence for such transformations;[22] indeed, as Holmes says, Helmholtz's experiment had risen to the purpose for which Liebig claimed to have written his book - to raise well-defined questions which would stimulate experimentation along the boundary of chemistry and physiology.[23]

Although Helmholtz thus vindicated one of Liebig's ideas, he realized then and for the rest of his life that it was only a partial vindication. Liebig already believed beyond any possibility of doubt that muscle activity was equivalent and correlatable with the chemical transformations underlying it - namely, that work and chemical force were correlated and, moreover, that the net *Kraft* in this process was conserved. Helmholtz realized that this was an article of faith which, though acceptable as a basis for his own as well as Liebig's research, was still unproven; and with his caution and requirement for testing bright ideas before admitting them as true, precepts which he had acquired from Müller, he continued to be on his guard against Liebig's enthusiasm. This interpretation explains why, for instance, in a paper that Helmholtz read before the Royal Society of London in 1861 on 'The application of the law of conservation of force to organic nature' he never mentioned Liebig, even when discussing the research that had been done on animal heat and muscle activity. There he declared that:

> "As yet, we cannot prove that the work produced by living bodies is an exact equivalent of the chemical forces which have been set in action. It is not yet possible to determine the exact value of either of these quantities ...."[24]

Liebig had declared that such a proof had been found in the early 1840s but, we may surmise, Helmholtz, appreciating that that declaration had been an article of faith rather than a sober scientific conclusion, considered it not worthy of mention. At the end of the 1861 paper Helmholtz did admit that the equivalence between animal motion and chemical force might yet be proved - by designing experiments in the light of his own rigorous, mathematical law of the conservation of force.[25] (Such a method, he well knew, had always been foreign to Liebig.)

Helmholtz's work on muscle led him directly into the question of animal heat. In his *Animal chemistry* Liebig had asserted that the entire heat an animal generates comes from the chemical energy within its food released during respirative combustion (see Chapter 14), despite the inability of Dulong's and Despretz's experimental research to account for more than about 80% of the heat in this way. Liebig had ridden rough-shod over their results, but in 1845 he presented a reasonable solution to their difficulty. Dulong had based his calculations on Lavoisier's heat-values of the direct combustion of carbon and hydrogen, whilst Despretz had measured his own values. Liebig maintained that measurements of direct combustion of these elements were unreliable for technical difficulties, and he had done indirect measurements instead - by measuring the combustion heats of olefiant gas, alcohol and ether and substracting the heat presumed to be due to the hydrogen in each compound. With these new figures he redid Dulong's and Despretz's calculations, thus obtaining a better fit between the calculated and measured animal heats. In his opinion, all discrepancy had been eradicated.[26]

However, not everyone was convinced and Helmholtz was one of those who pursued the problem. The average of Liebig's new ratios was 0.95 and some of his assumptions, as Helmholtz pointed out, were exceptionable. Helmholtz also showed that the basic source of all these discrepancies had begun with Lavoisier's hypothesis of respiration, whereby the carbon and hydrogen of the respirative substrates were supposed to exist as separate materials in the lungs. Dulong, Despretz and Liebig had accepted this supposition, never doubting that the heat of combustion of a nutrient compound should equal the sum of the combustive heats of its separate elements. Unfortunately no experimentally-determined combustive heats of nutrients were available, but from a few measurements on similar compounds Helmholtz concluded that the actual heat of combustion could be far different from the value based on Lavoisier's supposition. Hence, Dulong's and Despretz's results did not disprove the chemical theory of animal heat.[27]

So much for the experimental details of Helmholtz's research on animal heat. Woven into his second paper of that year, '*Bericht über die Theorie der physiologischen Wärmeerscheinungen*', were the deeper philosophical and physical implications of this research. The second paragraph began with his belief that Liebig's work on animal heat "offers more of physical interest /than of merely physiological/, in so far as it is a question of deciding whether animal heat arises from the known causes of heat production of inorganic nature ...."[28] In other words, it was an issue of universal significance. That significance was spelled out about a third the way through the paper, where he acknowledged the work already done on force-equivalents by other researchers:

> "We find ourselves, with the question of animal heat, in a special field. The principle of the constancy of the equivalence of force by the arousing of one natural force through another, although completely justified by logic, and also already utilized as a basic premiss of mathematical theories, /here he cited Carnot, Clapeyron and Neumanns/, has never been fully theoretically enunciated and recognized, norempirically demonstrated, if the researches done so far are fully representative of it. So long as a heat-substance was firmly held to be the basis of the phenomena, it was inconceivable that this substance could be generated within the body.
>
> But at present the material theory of heat is no longer held, and in its place a theory of motion is substituted, for we observe that heat is derived from mechanical forces, as well directly, for example by friction ..., as also indirectly through electrical currents produced by the motion of magnets, and through frictional electricity, where one cannot imagine a release of latent heat-substance. If we consdier heat as motion, it is a foremost point that mechanical, electrical and chemical forces can always produce only a certain equivalence of it, regardless of how complex the transformation of the one force into the other may be".[29]

Therein was his programme for the next two years' work on force. Holmes has asserted that it was clearly through the issues raised by Liebig that Helmholtz perceived the need for the thorough study which yielded his famous paper on the conservation of energy in 1847.[30] With this I agree, but with a qualification: we must not ignore Müller's realization of the importance of animal heat and physiological dynamics; we have seen what Müller wrote about these issues in his *Handbuch* and we can see how Liebig's and Helmholtz's work followed so neatly from his ideas. Nor should we forget that it was Müller who put Helmholtz onto this field in physiological chemistry in the first place.

Late in 1845 Helmholtz was asked to contribute an article on animal heat to the *Encyklopädische Handwörterbuch der medicinischen Wissenschaften*, published by the medical faculty of Berlin. That article, entitled '*Wärme: physiologische*',[31] (which students of the emergence of the energy conservation principle have invariably not mentioned), contained all his recent conclusions about animal heat, but more besides. He argued that from Hess's law and Lavoisier's principle of the conservation of matter, the quantity of heat in Nature must be constant; that the heat of an organism must therefore come ultimately from without itself, and that since the only discernible income for any organism is its food and oxygen, its heat production must come from the latent heat of its food. This was a novel and (be it noted especially) a theoretical demonstration of the chemical theory of animal heat, which Müller had only suggested and Liebig had only believed in but never proven. Unlike Liebig's intuitive belief, it rested on quite unassailable grounds. Continuing his theoretical line, Helmholtz declared that the only alternative was to consider organisms as loci of a special force, by which natural forces and heat could be generated *ad infinitum* - an hypothesis which contradicted all known laws in mechanics. Here, however, he seemed to encounter an unscalable obstacle to his line of reasoning, for he felt bound to admit that a contradiction of the mechanical laws could not theoretically be objected to if physiologists chose to assume that such an enigmatic force characterized the living state.[32] In other words, physiologists were free to deny any *a priori* reason for the identity of physical and vital forces.

How and when did Helmholtz overcome this last dilemma ? Did he overcome it by the time he wrote his 1847 memoire? If not, this explains why that memoire was a purely mathematical and theoretical one - essentially in the same vein as the essays on *vis viva* by the 18th century mathematicians - and cannot by any means be called a physiological approach to force. Admittedly, Helmholtz declared in a lecture in 1891 that he had written it "to present the facts for the benefit of physiologists";[33] but that he scarcely mentioned even the word physiology in that memoire shows that he had not yet resolved the dilemma of vitality.

In October 1846 Helmholtz sent a '*Bericht ueber die Theorie der physiologische Wärmeerscheinungen betreffende Arbeiten ... für 1845*' (Report on the work done on the theory of animal heat for 1845) to the *Fortschritte der Physik*, the journal of the *Physikalische Gesellschaft*.[34] This report was essentially an abstract of his 1845 Encylopaedia article but, as Müller had taught, he had been gradually refining his ideas and they now anticipated more closely his 1847 paper. The main conclusion of the report was that the kinetic theory of heat implied that mechanical, electrical and chemical forces must be the equivalents of a single, fundamental type of force subsuming all others, and that the confirmation of this conclusion was the empirical task of physicists and physiologists.

Throughout the rest of 1846 he experimented on heat evolved by active muscle, frequently discussing his ideas with du Bois-Reymond. He was also formulating the general significance of his work in writing, and in February 1847 sent du Bois-Reymond a sketch of an introduction to a proposed memoire on force:

> "... not because I think it is ready, for even in reading it over I see that most likely none of it can stand, but because I do not yet see how many times I shall have to rewrite it before it is done, and I want to know if you think its style will go down with the physicists".[35]

Du Bois-Reymond declared it had to remain as it was, calling it "an historical document of great scientific import for all time".[36] What du Bois-Reymond did not see was the depth of Helmholtz's dissatisfaction with it, for the problem he had hoped to solve conclusively, namely the dynamics of life, was still unresolved. That paper, read to the *Physikalische Gesellschaft* on June 23rd 1847 and so enthusiastically received by its members, must have been in its author's own opinion an admission of failure. Its author was, after all, a physiologist, and he had failed to solve a physiological problem. He had not even started on the empirical verification of the principle which, as we saw above, he had urged physicists and physiologists to do. Indeed, it is not surprising that when he sent his paper to Magnus, asking him to arrange for its publication in *Poggendorffs Annalen*, Magnus objected to its abstract and mathematical character, although he recognized the significance of its argument. In 1871 when Helmholtz delivered his 'In memoriam' on Magnus, he generously acknowledged the wisdom of Magnus's standpoint:

> "I must confess that I myself and many of my companions formerly thought that Magnus carried his distrust of speculation too far, especially in relation to mathematical physics. ... yet when we look around us from the standpoint which science has now attained, it must be confessed that his distrust of the mathematical physics of that time was not unfounded. At that time, no separation had been distinctly made as to what was empirical matter of fact, what mere verbal definition, and what only hypothesis. The vague mixture of these elements was put forth ... as axioms of metaphysical necessity and postulated a similar kind of necessity for the results".[37]

As an instance of the excessive rôle that hypotheses had played in science he mentioned the theories that had been promulgated recently on atoms. He did not mention the metaphysical ideas on the unity of forces, of which he must have heard. Why this omission I cannot imagine; but this is a useful place to examine Helmoltz's own epistemology as a prelude to examining some details of his 1847 paper.

Helmholtz's epistemology during the 1840s and 1850s

Helmholtz often asserted the relevance of philosophy to a scientist's understanding not only of the world, but also of the way he sees the world. The Kantian distinction between noumena and phenomena was always in his mind, Kant's influence being discernible throughout his life's work. In his physiological studies, particularly of sense-perceptions on which he wrote voluminously in the 1850s and 1860s,[38] he believed he was actually verifying and developing Kant's epistemology; nowhere was this clearer than in the law from which he started - Müller's law of specific nerve energies, which had shown that man's knowledge of the external world is fashioned inescapably by the constitution of his sensory organs. Helmholtz's problem was therefore to decide how man can have any worthwhile knowledge of the world. The way out was by following Kant in insisting that the law of causality, which is the highest form whereby man relates his sense-data one to another, is transcendental and *a priori*. (Other categories Helmholtz thought *a priori* were the conservation of matter and the concepts of time and space).[39] Such ideas had been in his mind even during his schooldays; as he once admitted:

> "My interest in questions raised in the theory of knowledge was implanted in me in my youth when I often heard my father, who was strongly influenced by Fichte's idealism, argue with those of his colleagues who favoured Kant or Hegel".[40]

Although Helmholtz's interest in philosophy had an early start, his distaste for metaphysics set in not much later. In a lecture in 1855 he proposed that the causal law underlies man's belief in external things, a proposition which drew charges of plagiarism from the metaphysician Arthur Schopenhauer (1788-1860)[41] and confirmed his aversion to contemporary metaphysics - by which he meant excessive reliance on the deductive method, with no regard for its empirical basis or its limitations. In an address in 1877 he declared that during his student days he had suffered under an extreme spiritualistic metaphysic, and that Kant had been the only salvation: his *Kritik der reinen Vernunft* was "a continual sermon against the use of the category of thought beyond the limits of possible experience".[42] Two of Kant's *a priori* categories that he found especially useful were matter and force, for into them could be fitted all external things. Whether such things actually exist he answered with a frankness that reminds one of Müller: it was a question purely for metaphysics and would always be so, since idealism and realism were equally self-consistent. However, the invariability of matter and force depended on the assumption that Nature is lawful, which in turn relied on the *a priori* causal law.

It is worth emphasizing the rôle of the causal law in Helmholtz's philosophy for, although it seems incontestable to most people today, it was at that time generally recognized as a strategic command post that had to be won and defended in the battle for the right philosophy and the right method for investigating natural phenomena. Helmholtz handled it in several ways, one of the most interesting in my opinion, (for this did not rely particularly on Kant), being the approach via the question of general relations; this occurred in a paper on '*Die neueren Fortschritte in der Theorie des Sehens*' (1868), (The recent progress of the theory of vision), where he declared that Müller's law led to the realization that the only features of reality that man can know are the relations among its phenomena:

> "But if what we call a property implies an action of one thing on another, then a property or quality can never depend upon the nature of one agent alone, but exists only in relation to, and depends on, the nature of some second object, which is acted upon. Hence there is really no meaning in talking of properties of light which belong to it absolutely .... The notion of such properties is a contradiction in itself ....
>
> These considerations have naturally long ago suggested themselves to thoughtful minds; they may be found clearly expressed in the writings of Locke and Herbart; and they are completely in accordance with Kant's philosophy. But in former times they demanded a more than usual power of abstraction...."[43]

The paramount of these relations was causality. (Let us recall that the importance of Locke's analysis of 'relations' and 'causality' was a main argument in Chapter 1 of this thesis).

There was much more to Helmholtz's philosophy, particularly as it developed in his later years.[44] But the outline above must suffice for the two purposes I have had in mind: 1) To show that he was deeply concerned with philosophy, particularly with Kant and with the need to reconcile transcendentalism and empiricism, and that his philosophy was woven into his scientific work. And 2) To provide an adequate background for discussing the philosophical content of his 1847 paper.

Helmholtz's paper '*Ueber die Erhaltung der Kraft*' begins as if it is essentially a *jeu d'esprit*. That is not to say that it was not meant to be taken seriously, only that it was not solemn or unimaginative. Its opening reminds me of the nonchalance to be found in several other of the seminal works in natural philosophy - in Plato's *Timaeus*, in Copernicus's own preface to his *De revolutionibus*, in Galileo's *Dialago*. It seems to say "Let's pretend so and so, then see what will pop up". One almost gets the impression, as indeed many men did from the *Timaeus*, *De revolutionibus* and *Dialogo*, that the author might not be too worried if his bright idea might not stand up to comparison with the real world, for its style is that of a philosophical adventure and nothing so prosaic as a mere account of things as they seem to be. Perhaps what I have just said seems hardly worth saying; yet we must remember that in one respect Helmholtz's essay did not touch the real world - it did not solve his dilemma over vitality.

Helmholtz's approach to his subject began not with any statements of fact but with two sets of assumptions. Firstly, he supposed matter to consist of ultimate particles, whose motions and the phenomena they produce depend solely on forces akin to gravititation emanating from their centres, whose actions occur along straight lines joining the centres of such particles, and whose intensities depend only on distance. Then, employing the laws of motion of gross matter,and also assuming that all forms of energy (*Kraft* being the usual word employed), depend upon the motions or positions of particles, the conservation of energy would hold and perpetual motion would be impossible under any circumstances whatever.[45]

The second set of assumptions went thus: assume perpetual motion to be impossible, and accept Newton's third law of motion that action and reaction are always equal and opposite; then a series of mathematical deductions leads to the conservation of motion.[46]

Underlying both approaches was the law of causality which Helmholtz did not see any need to defend. That much we can grant him, but there were profound problems with his two initial sets of assumptions which he did not discuss and which were occasions of trouble for his paper's reception.

The assumption that the only forces in the world are central and akin to gravitation was questionable, and Helmholtz was probably already a skillful enough mathematician to realize it. Moreover, that assumption would have been difficult to reconcile with Boscovich's and others' theories, since it took no account of repulsive forces or of forces which might not follow an inverse square. Moreover, did he really believe that particles (hard, impenetrable and extended) existed? After all, they were hardly reconcilable with the mathematical philosophies of Boscovich and Leibniz. Or did he mean by particle a centre of force? Some years later Clark-Maxwell thought he meant the latter and pointed out its difficulties: how can a 'nothing' resist motion? How can it possess mass and inertia?[47] Helmholtz must have been aware that such questions had been put to Boscovich and Leibniz themselves, and his silence seems to emphasize his awareness of the unprovability of such assumptions. Emile Meyerson has given a useful account of how these difficulties were handled in the late 19th and early 20th centuries,[48] and although that account does not bear on Helmholtz's own thoughts in 1847 it has this one use - that we can see how the philosophical ambiguities in his paper refused to lie down. Helmholtz himself was content not to discuss them, probably because he believed Kant had dealt with them generally, for instance by asserting the *a priority* of matter and force, and because they would have occupied excessive space in his paper.

As for his set of assumptions based on the impossibility of perpetual motion, that had a history of which he was well aware and which might be outlined here in so far as it is relevant. We have already mentioned the analysis of *vis viva* and the conservation of motion by 18th century mathematicians. There had been other well known discussants of perpetual motion: Leonardo da Vinci had argued its impossibility;[49] so had Girolamo Cardano (1501-1576) the mathematician.[50] Galileo had declared that such a motion could not be created by means of simple machines;[51] whether he considered this to be an universal principle, or only a characterization of machines below a certain level of sophistication, is disputable. A little later, the mathematician Simon Stevinus (1548-1620) used the reverse of Galileo's argument; taking the impossibility of perpetual motion as self-evident, he deduced rules for the operation of machines.[52] As we have seen, Leibniz used the principle to demonstrate the conservation of *vis viva*, and Huyghens used it likewise. Sadi Carnot (1796-1832) used it as the basis for his *Réflexions sur la puissance motrice du feu* (1824),[53] and in 1839 Marc Séguin began with it in his analysis of force and heat. To this line, though not in its entirety for he was still un-aquainted with some of it in 1847, Helmholtz saw himself as adding another weighty argument. However, it is important to realize the different statuses that the impossibility of perpetual motion had had for these different men. For Leibniz it had been a simple corollary to the causal principle, since perpetual motion would necessitate an effect greater than its cause.[54] Huyghens was more cautious; he believed its impossibility to be established by *a posteriori* theoretical demonstration for purely mechanical systems, but he was not sure about other systems, "as in the employment of the magnet, there may be some hope".[55] Carnot was emphatic on the issue: having described a hypothetical system which

> "would be not only perpetual motion, but an unlimited creation of motive power without consumption either of caloric or of any other agent whatever,"

he added

> "Such a creation is entirely contrary to ideas now accepted, to the laws of mechanics and sound physics. It is inadmissible".[56]

For Helmholtz, it seemed to be an experimental truth since he mentioned how fruitless the many empirical searches for a *perpetuum mobile* had been and that in 1775 it had been proscribed officially by the Paris Académie des Sciences.[57] The Académie had announced that in future it would not examine "any solution of the problems of the duplication of the cube, of the trisection of the angle, or the quadrature of the circle, nor any machine announced as perpetual motion".[58] (Ironically, this was the very year when the Bavarian Academy of Sciences announced a title for a prize-essay on whether perpetual motion could occur in the living organism. See my chapter on Roget.) Actually, as Helmholtz well knew, a succession of negative results did not constitute a positive demonstration against perpetual motion. His argument was therefore unrigorous. But that did not matter as it accorded perfectly with the epistemology announced at the very start of his essay:

> "The task of the physical sciences is to discover laws so that individual natural processes can be traced back to, and deduced from, general principles. These principles, such as the laws of refraction and reflection of light and the laws of Mariotte and Gay Lussac about the volumes of gases, are obviously nothing but generic concepts through which the phenomena falling under them are collectively understood. The search for such laws is the task of the experimental part of our sciences. The theoretical part, on the other hand,seeks to ... comprehend them according to the law of causality".[59]

It would not be unreasonable to say that he regarded the impossibility of perpetual motion as a generic concept - that is, as an empirical principle. But from his mentors, Kant and Müller especially, he knew the fallibility of empirical generalizations. Yet here he was, in a philosophical essay, basing his demonstration of the conservation of energy on one such fallible generalization! How could he be so inconsistent?

I have searched the secondary literature on this point and have found nothing. Perhaps I am imagining smoke when there is really no fire. Perhaps Helmholtz would say that I am behaving like a metaphysician. Yet to base a demonstration of a universal law on a shaky foundation cannot be dismissed. (Meyerson dissected his argument on perpetual motion and found it wanting,[60] but that has nothing to do with my point here.)

However, there are two plausible explanations in Helmholtz's own later writings. In a lecture '*Ueber die Wechselwirkung der Naturkräfte*' (On the interaction of natural forces) in 1854 he described how, until the previous century, the *perpetuum mobile* had been a commonly sought prize, and that the type of question asked was how to combine the natural forces so as to produce it. Latterly, however, the question had been inverted: if perpetual motion be impossible, what are the relations which must subsist between natural forces? The justification for this inversion was thus not a philosophical one. It merely made things easier:

> "Everything was gained by this inversion of the question. The relations of the natural forces, rendered necessary by the above assumption, might be easily and completely stated".[61]

So the neatness of the result justified the preliminary assumption against perpetual motion.

The second explanation is less plausible as it derives from a lecture he gave in 1891, and at seventy years of age a man's recollections are suspect. There he described how, having become dissatisfied with Stahl's *anima*, he began to ask

> "What relations must exist among the various natural forces for perpetual motion to be *possible*? And do these relations actually exist? In my memoire 'The conservation of force', my aim was merely to provide a citical examination of these questions ...."[62]

So it seems that the original question about perpetual motion was the inverse of the question he actually asked in his 1847 memoire. Why? Could it be that the answer he found to the original question suggested the existence of dynamical relations which were not known to mechanical philosophers, but might be possible within living organisms? Such an answer would not have been acceptable to his colleagues in the *Physikalische Gesellschaft*. Besides, it would have been useless, since it brings one back to asking the original question which had apparently made him dissatisfied with Stahl. By contrast, if one begins by assuming the impossibility of perpetual motion, a much neater answer emerges, for, amongst other reasons, a universe of conserved energy or motion is bound to be *simpler* than one generating its own source of motion continuously; the difference between the two is, to modify a phrase from Koyré, rather like going "from a closed world to an indefinite universe."

All this supports a common contention among historians of ideas - that most, if not all, of the primary ideas underlying science have been intuitive and have been employed without scrupulous analysis of their philosophical validity. As Meyerson said of the doctrines of inertia and of the conservations of matter and energy,[63] so might we say of Helmholtz's and others' ideas on the impossibility of perpetual motion: "it is neither empirical nor *a priori*; it is plausible." Helmholtz would probably have disagreed with this, for if ever there was a scientist (in the modern acceptation of that word), the warp and woof of whose work was philosophical and carefully rationalized, it was he. Of this, his treatise *Physiologische Optik*[64] (3 vols., 1856-1866) was a magnificent example; especially in volume III, the text was interspersed with frequent appeals to Leibniz, Spinoza and Kant, Locke, Hume and other empiricists, but rarely to the *Naturphilosophen*.

It was a colossal treatise straddling two worlds - the old, in its discursiveness and polymathic erudition; and the new, in the elegance and penetration of the experiments and mathematics it employed. It is not surprising that even in 1924-25 it was published in America, not merely because it had become a classic, but also because it was still thought "a model of scientific method and logical procedure" and because "the demand for the book has not ceased, and will not cease for a long time to come, for no new treatise has superceded Helmholtz's work".[65]

Regrettably we have no space here to discuss his work in optics, not even how it employed and extended his ideas on force, for that would take another chapter. Instead, we should conclude by asking what was the reception given to his 1847 paper. We have seen Magnus's attitude. Helmholtz himself said

> "I was quite prepared for the experts to say simply 'We know all that. What is this young doctor thinking about, who considers himself called upon to explain it all to us so fully?' To my astonishment, however, the authorities on physics with whom I came in contact received it quite differently. They were inclined to deny the correctness of the law, and because of the heated fight in which they were engaged against Hegel's philosophy of Nature, to treat my essay as a fantastic piece of speculation. Only the mathematician, Jacobi, recognized the connection of my line of thought with that of the mathematicians of the previous century ...."[66]

Poggendorff refused to publish it in his *Annalen* because it was too theoretical. Du Bois-Reymond took it to his publisher, Georg Reimar, then engaged in publishing his papers on animal electricity; Reimar not only published it but also gave Helmholtz an honorarium, a financial recognition rarely awarded for such an abstruse work. The younger members of the *Physikalische Gesellschaft* enthused over it; as du Bois-Reymond remarked:

> "His supporters declared that he had set in motion the conservation of another force, much more interesting for us, the mind of Helmholtz himself".[67]

Yet they misunderstood Helmholtz's original aim and how he had failed to elucidate the dynamics within the living organism. They enthused because his paper was so elegant and conclusive within the world-view they had defined for themselves, a world-view which was as intellectually confined as that of the *Naturphilosophen* had been limitless. Du Bois-Reymond described it in 1842 thus:

> "Brücke and I pledged a solemn oath to put in effect this truth: no other forces than the common physico-chemical ones are active within the living organism. In those cases which cannot at the time be explained by these forces, one has either to find the specific way or form of their action by means of the physico-mathematical method, or to assume new forces equal in dignity to the chemical-physical forces inherent in matter, reducible to the forces of attraction and repulsion".[68]

As we have seen, such a philosophy was alien to their teacher, Müller. And though they probably did not realize it, it was uncomfortable for Helmholtz and became increasingly so as his success in experimental science grew. In a lecture in 1854, republished unmodified in 1884, where he described the emergence of the energy conservation doctrine, having acknowledged Mayer as the first to appreciate and express the doctrine correctly, he asked how it might apply to the motions and work of organisms. Liebig he acknowledged for giving one answer to this in the form of his theory of nutrition. Yet he had to admit that

> "... we are in possession of no experiments from which we might determine whether the *vis viva* of the sun's rays which have disappeared corresponds to the chemical forces accumulated [in plants and animals]. If this view should prove correct, we derive from it the flattering result that all force, by which our bodies live and move, finds its source in the purest sunlight; and hence we are all, in point of nobility, not behind the race of the great monarch of China, who heretofore called himself the Son of the Sun".[69]

What a grand idea, yet how unstable. Helmholtz knew how far he was from demonstrating it empirically. In the meanwhile, the best he could do was to cling to what he considered the true epistemology - essentially Kant's, and employ as many experimental tools as possible in its service - as Müller had taught him. To use one of his own favourite quotations:

> "They imagined they knew what they did not know, and he at any rate had the advantage of not pretending to know what he did not know."

NOTES TO CHAPTER 16.

1. As every student of Helmholtz must be, I am endebted to the 3 volume biography by Leo Koenigsberger, *Hermann von Helmholtz*, Braunschweig, Friedrich Vieweg, Erster Band 1902, Zweiter Band 1903, Dritte Band 1903. In addition, I could not have done without: Emile Meyerson, *Identité et realité*, first published 1908. I have used the English translation by Kate Loewenberg, *Identity and reality*, London, Allen & Unwin, 1930, and found chapters 1, 2, 4, 5, 8, 9 and 10 particularly useful. Gerd Buchdahl, *Metaphysics and the philosophy of science*, Oxford, Blackwell, 1969. Yehuda Elkana, *The discovery of the conservation of energy*, London, Hutchinson Educational Ltd., 1974, chapters 4 and 5. Russell Kahl, *Selected writings of Hermann von Helmholtz*, Connecticut, Wesleyan University Press, 1971.

2. H. von Helmholtz, 'On thought in medicine', in *Lectures on scientific subjects*, trans. by E. Atkinson, London, Longmans Green, 1891, pp.202-203.

3. Perhaps the most succinct and classic discussion of this is O. Temkin, 'Materialism in French and German physiology in the early 19th century', *Bull.Hist.Med.*, 1946, 20:322-327.

4. K. Rothschuh, *A History of physiology*, trans. by G.B. Risse, New York, Robert Krieger, 1973, chapter 6.

5. J.G. McKendrick, *Hermann Ludwig Ferdinand von Helmholtz*, London, Fisher Unwin, 1899, p.22.

6. Helmholtz, *op.cit.*, Note 2 above, p.222.

7. *Ibid.*, p.223.

8. H. von Helmholtz, 'An autobiographical sketch', 1891. Translated in full in Kahl, *op.cit.*, Note 1 above, pp.466-478, particularly p.471.

9. See W.A. Shenstone, *Justus von Liebig, his life and work*, London, Cassell, 1895, chapter 5, esp. pp.65-66. See also J.S. Fruton, *Molecules and life. Historical essays on the interplay of chemistry and biology*, New York, John Wiley, 1972, p.47.

10. Shenstone, *ibid.*, p.66.

11. In addition to von Meyer's and Fruton's studies, I have found useful the papers by M. Teich, 'The historical foundations of modern biochemistry', and by M. Dixon, 'The history of enzymes and of biological oxidations', in J. Needham, (editor), *The chemistry of life*, Cambridge, C.U.P., 1970, pp.171-191 and 15-37 respectively.

12. J. Liebig, '*Ueber die Erscheinung der Gähnung, Fäulnis und Verwesung und ihre Ursachen*', *Liebigs Ann.*, 1839, 30:250-287. This is discussed by Fruton, *op.cit.*, Note 9 above, pp.47-48. Also by Dixon, *op.cit.*, Note 11 above, pp.19-20.

13. H. Helmholtz, '*Ueber das Wesen der Fäulness und Gährung*', cited in Koenigsberger, *op.cit.*, Note 1 above, p.53, and in McKendrick, *op.cit.*, Note 5 above, p.28.

14. Koenigsberger, p.54.

15. *Ibid.*, p.51. Also *op.cit.*, Note 8 above, pp.470-471.

16. For a summary of Stahl's ideas, I have used T.S. Hall, *Ideas of life and matter*, Chicago, Chicago University Press, 1969, vol.1, pp.351-366.

17. *Op.cit.*, Note 8 above, pp.470-471.

18. Meyerson, *op.cit.*, Note 1 above, p.192.

19. H. Helmholtz, 'The application of the law of the conservation of force to organic nature', an address to the Royal Society of London, 1861. Translated in full in Kahl, *op.cit.*, Note 1 above, pp.109-121. See particularly pp.115-116.

20. *Ibid.*

21. Holmes, *op.cit.*, Note 3, chapter 14, above, p.LXXVIII. The title of Helholtz's paper was '*Ueber den Stoffverbrauch bei der Muskelaktion*', discussed too in Koenigsberger, p.59.

22. See for example, D.L. Hermann, *Elements of physiology*, trans. by A. Gamgee, London, Smith,Elder & Co., 1875, pp.250-251.

23. *Op.cit.*, Note 21 above, p.LXXIV.

24. *Op.cit.*, Note 19 above, p.119-120.

25. *Ibid.*, p.121.

26. Cited in Holmes, *op.cit.*, Note 21 above, p.LXXV.

27. *Ibid.*, p.LXXV.

28. H. Helmholtz, '*Bericht ueber die Theorie der physiologischen Wärmeerscheinungen für 1845*', in *Die Fortschritte der*

Physik im Jahre 1845, Berlin, Physikalische Gesellschaft, 1847, pp.346-355. Reprinted in H. Helmholtz, *Wissenschaftliche Abhandlungen*, vol.1, Leipzig, J.A. Barth, 1882, pp.3-11. This quotation is from p.4 of the reprint.

29. *Ibid.*, pp.6-7.

30. H. Helmholtz, '*Ueber die Erhaltung der Kraft*', translated in Kahl, *op.cit.*, Note 1 above, pp.3-55.

31. Koenigsberger, p.60.

32. *Ibid.*, p.61.

33. *Op.cit.*, Note 8 above, p.471.

34. Koenigsberger, p.64.

35. *Ibid.*, p.68.

36. *Ibid.*, p.68.

37. H. Helmholtz, 'Gustav Magnus, In Memoriam', in *Popular lectures on scientific subjects*, 2nd series, trans. by E. Atkinson, London, Longmans, Green & Co., 1884, pp.1-26. This quotation, p.16.

38. See Koenigsberger, vol.1, pp.VIII-XI, and Vol.2, pp.VII-IX for an invaluable bibliography.

39. See Kahl's introduction, *op.cit.*, Note 1 above, and Helmholtz's two papers on 'The origin and meaning of geometric axioms', pp.246-265, and pp.360-365 in Kahl's compilation.

40. *Op.cit.*, Note 8 above, p.476.

41. These charges were almost certainly untrue. Schopenhauer seems to have had a cantankerous disposition and was not infrequently engaged in similar controversies. Helmholtz would not have considered him a profound enough philosopher to appropriate his ideas.

42. *Op.cit.*, Note 2 above, p.230.

43. This paper is translated in R.M. & R.P. Warren, *Helmholtz on perception: its physiology and development*, New York, John Wiley, 1968, pp.61-136. This quotation, p.102.

44. Koenigsberger is an invaluable source. Welby seems to present Koenigsberger's discussions of Helmholtz's philosophy accurately. For a succinct, but useful, account see the article on Helmholtz by R.S. Turner in *Dictionary of Scientific Biography*, New York, Charles Scribner's Sons, 1975, vol.6, pp.241-252.

45. Helmholtz, *op.cit.*, Note 30 above, pp.8-16, particularly.

46. *Ibid.*, p.7 especially.

47. Cited in McKendrick, *op.cit.*, Note 5 above, p.48.

48. *Op.cit.*, Note 1 above, Meyerson's chapter on 'Mechanism' in particular, pp.63-112.

49. Cited in *ibid.*, p.202. See also P. Duhem, *Les origines de la statique*, Paris, A. Hermann, 1905-1906, vol.1, p.21.

50. *Ibid.*, p.202. And Duhem, p.58.

51. *Ibid.*, p.202.

52. S. Stevin, *Oeuvres mathématiques*, trans. by A. Girard, Leyden, B. & A. Elsevier, 1634, 6 vols. in 1, p.448; cited in *ibid.*, p.202.

53. S. Carnot, *Réflexions sur la puissance motrice du feu* (1824), reprinted in E. Mendoza (editor), *Reflections on the motive power of fire, and other papers on the second law of thermodynamics by E. Clapeyron and R. Clausius*, New York, Dover, 1960, p.12.

54. See my Chapter 10 above.

55. Leibniz, *Mathematische Schriften*, edited by C.I. Gerhardt, vols. 1 & 2, Berlin, A. Asher & Co; vols. 3-7, Halle, H.W. Schmidt. This citation vol.1, p.419, cited in Meyerson, p.203.

56. *Op.cit.*, Note 53 above, p.12. See also his footnote.

57. Meyerson, p.204.

58. *Ibid.*

59. Helmholtz, paper in Kahl, *op.cit.*, Note 1 above, p.3.

60. Meyerson, pp.203-205 in particular.

61. H. Helmholtz, 'On the interaction of the natural forces', in H. Helmholtz, *Popular lectures on scientific subjects*, series 1, trans. by E. Atkinson, London, Longmans, Green & Co., 1884, pp.137-174. This quotation, p.148.

62. *Op.cit.*, Note 8 above, p.471.

63. Meyerson, p.206.

64. H. Helmholtz, *Handbuch der physiologischen Optik*, Leipzig, L. Voss, Erster Teil 1856, Zweiter Teil 1860, Dritte Teil 1867. I have used the translation of the 3rd edition by J.P. Southall, *Helmholtz's treatise on physiological optics*, 3 vols., Wisconsin, G. Banta, 1924.

65. *Ibid.*, translator'spreface.

66. *Op.cit.*, Note 8 above, p.471.

67. Cited in McKendrick, *op.cit.*, Note 5 above, p.44.

68. Cited in Kahl's introduction, p.XVIII.

69. *Op.cit.*, Note 61 above, p.165.

Epilogue

As is well known, there were other major contributors to the conservation of energy who have been scarcely mentioned in this thesis. I have been tempted to discuss, for instance, the work of Julius Robert Mayer, since he was a physician and it was a clinical observation, namely the unexpected redness of venous blood in sailors on whom he practised blood-letting in Java, which led him to ponder the relation between heat and work and to elaborate this into a universal principle of *Kraft*. But for me to have written anything worthwhile would have necessitated several months of research and would have lengthened this thesis excessively. Besides, if I were to include Mayer, why not Ludwig Colding, for he would have been an equally interesting example of a *Naturphilosophe*?

It might be useful to conclude on two brief points. The first is the differentiation we make today among the concepts of force, energy, work and motion, and which makes the principle of energy conservation precise and really meaningful. Undoubtedly, the concept which emerged in Helmholtz's paper was what we today call energy - namely, the capacity a force possesses of moving its point of application; it is the capacity to do work. The word *Kraft* covered both our energy and our force. Actually, Helmholtz used various terms - *Arbeitskraft*, *bewegende Kraft*, *Spannkraft*, *mechanische Arbeit* and *Arbeit* for his fundamental, measurable force. Our modern force, despite what modern physics textbooks say, is still a *vis occulta* and is essentially the *deus* in almost any type of *machina*. The word *Energie* came closer to what we mean by energy, but as we saw from Johannes Müller's use of it, this is a refinement that an honest historian would find difficult to defend.

Such distinctions are critical today; our civilization would not be as it is if they had not been made. But they were made after the time-span of this thesis. William Clark Maxwell, who did much to refine these terms, said of that period that:

> "The fathers of dynamical science found a number of words in common use expressive of action and the results of action such as force, power, action, impulse, impetus, stress, strain, work, energy. They also had in their minds a number of ideas to be expressed, and they appropriated these words as best they could to express the ideas. The words force, *vis*, *Kraft* came most readily to hand."

We may safely say there was no real need felt for any rigorous distinctions. On the other hand, there had been a long-felt need for a conservation of something. This is my second point. Indeed, one is tempted to wonder why the phrenologists did not identify a 'conservation bump' in the human brain, for the history of this dream goes back indefinitly. Des Carte's principle of the conservation of motion, which was both scientific and theological, illustrates well the deep-rootedness and therefore antiquity of the conservation urge. Perhaps this is one reason why the *a priori* character of the conservation of energy was proclaimed by some philosophers - not all of them *Naturphilosophen* - almost from the day of its official enunciation.

It is interesting to note that this apparently constitutive, genetic trait in mankind has existed alongside the equally persistent, apparently genetic trait of its antagonist, namely the quest for perpetual motion. Like genuinely genetic traits - for instance, haemophilia or sickle-cell anaemia - such traits are not reflections of intelligence. As Duhem rightly remarked, perpetual motion continued to be sought despite the impressive onslaughts against it by men like Des Cartes, Leibniz, Huyghens and Helmholtz; and its seekers were not all fools. It has been, and will always be, a shadowy figure lurking in the deepest recesses of the human mind. It is a 'philosopher's stone' and history shows that man always needs one. Perhaps this explains why Volta's ideas on contact electricity were accepted without great fuss, although he explicitly mentioned the possibility of obtaining an endless supply of electrical force apparently from nothing.

If such be the nature of these great ideas, what are we to make of them today? Is the conservation of energy descriptive of the noumena, or even of the phenomena, of the world? It has never been proven by experiment. Or is it only a useful type of abacus? Weighty minds like Mach, Stallo, Poincaré and Duhem have wrestled with this problem and, need I add? have not come to a consensus of opinion. My own belief is that these issues are probably forever irresolvable. What I have attempted to contribute to them has simply been to corroborate other scholars' assertions that the doctrines of the correlation of forces and the conservation of energy emerged from several directions, one of which was physiology, and to uncover some of the hitherto unexplored undergrowth. Clearly, this particular direction is still a field for much more historical research.

The complexity of the problem of force within the living organism, both for historians of the subject and even for modern biophysicists and biochemists, has been expressed most beautifully in the high Jacobean prose of a 17th century physician. In his words may this thesis end:

> "For though wee christen effects by
> their most sensible and nearest causes,
> yet is God the true and infallible cause
> of all, whose concourse, though it be
> generall, yet doth it subdivide itself
> into the particular actions of everything,
> and is that spirit by which each singular
> essence not onely subsists, but performes
> its operation."

> "We are onely that amphibious piece
> betweene a corporall and spirituall essence,
> that middle forme that links those two together,
> and makes good the method of God and nature,
> that jumps not from extreames, but unites the
> incompatible distances by some middle and
> participating natures."

[Sir Thomas Browne, *Religio medici*, 1643.]